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PSYCHICAL RESEARCH MONOGRAPH NO. 1

EXPERIMENTS
IN
PSYCHICAL RESEARCH
AT
LELAND STANFORD JUNIOR UNIVERSITY

BY
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With a Foreword by DAVID STARR JORDAN, Chancellor Emeritus; an Introduction by Professor FRANK ANGELL, Head of Department of Psychology; and a Part by Professor LILLIEN J. MARTIN, Professor Emeritus of Psychology

From the Division of Psychical Research
Department of Psychology

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to

THOMAS WELTON STANFORD

Whose Wisdom in Providing Opportunities
for Scientific Investigation Has
Anticipated the Greatest Need
of Psychical Research

44056

FOREWORD

Science is human experience tested and set in order. It involves not alone the experience of the individual, but so far as may be, the accumulated or recorded experience of the race, of which the experience of the individual furnishes the basis of understanding. To enter the category of science, the data on which generalized results are based must be fully tested in order to eliminate personal equations whatever their form or origin.

In the investigation of the varied phenomena embraced under the term of "Psychical Research," as in any other department of knowledge, the Scientific Method is the sole instrument on which we can depend. To every apparent fact we must apply the tests of science: observation, experiment, logic, and instruments of precision. That the phenomena in this field are peculiarly baffling affords no ground for discouragement. By the methods of precision they are reducible to scientific order, and we may be sure that in this field as in any other we can safely follow wherever Truth shall lead. Genuine knowledge can never run counter to sound principles in human life.

But in this difficult borderland of psychology in which subjective and objective mental conditions are closely intertangled, the investigator finds it well to be cautious. Obvious explanations are seldom the true ones, and generalizations hastily drawn from them may check the growth of knowledge. In this field, perhaps above all others, the use of the "method of intuition" as an instrument of precision is sadly out of place. One supreme test of safety in generalization is the articulation of supposed facts with the knowledge already tested and organized by science.

The work in Psychical Research at Stanford University has rested from the first on "the solid ground of nature." At the present stage, its methods seem more important than its results, although the latter, while not sensational, are unquestionably substantial.

DAVID STARR JORDAN.

To believe is dangerous, to be unbelieving is equally so; the Truth, therefore, should be diligently sought after, lest that a foolish opinion should lead you to pronounce an unsound judgment.—PHÆDRUS: *Fables,* Book III, 10:1 and 5, 6.

Hardly as yet has the surface of the facts called "psychic" begun to be scratched for scientific purposes. It is through following these facts, I am persuaded, that the greatest scientific conquests of the coming generation will be achieved. *Kühn ist das Mühen, herrlich der Lohn!*—WM. JAMES: *Final Impressions of a Psychical Researcher,* 1909, in *Memories and Studies,* New York, 1911, p. 206.

AUTHOR'S PREFACE

In a time when men's minds, scholars' minds, have been turned from philosophy to science, from principles implicit in human experience to principles empirical and eclectic, it is a strange anomaly that principles of life which are vital enough to determine men's conduct in their most serious concerns, and which are prevalent enough to be continuously operative in every civilized and uncivilized portion of the globe, are at once hailed by a small but important part of the learned world as the veritable principles of life, challenged by another equally important part of the learned world as groundless, and ignored by the great body of the responsible men of science as unworthy of that rigorous inspection by which alone principles based upon the phenomena of the world may win the imprimatur of scientific confirmation or refutation.

The sheer universality of human interest in, and human allegiance to, one or another of the principles based upon psychical, or other "alleged" phenomena, now classified in the field of Psychical Research, should confer upon these phenomena the right to continuous serious scientific investigation regardless of the lack of promise which it seems to the general body of the men of science to offer. It is no adequate defense to claim that science has no time to go out of its way to combat the superstitions and prejudices of men; for no matter to what extent superstition and prejudice may be supported by these alleged phenomena, the phenomena are initially accepted because it is believed they have been repeatedly observed by trustworthy, even eminently qualified, observers.

Now that university education is shared by an increasingly large proportion of the people in civilized countries, and scientific knowledge is being widely disseminated, the obligation of science to the public, in respect to these matters, is heavy and is becoming increasingly greater. It is to be hoped that the situation will now improve, and that other centers of learning will also assume this obligation and thereby make coöperative investigation possible.

The experiments described in this monograph fall into several classes of investigations which are fairly closely related to each other, and which are believed to be of fundamental importance to Psychical Research. They are offered as some slight contribution to science, of interest particularly to those who are more or less technically familiar with Psychical Research; possibly their less technical portions may inter-

est the layman. Lest the latter, however, be disappointed in finding no brief either for or against the general phenomena in this field, the assurance must hereby be offered him that the research is undertaken with a zeal for Truth, and is projected and controlled with an anxiety for the strength of the bridge it is building, which must bear the strain of the passage of men of learning, men of influence, men of science, from the shore of accepted knowledge to the island of the not-yet-recognized. Safety forbids bias or precipitancy. This laboratory report completes the first stage of construction.

Herein will be found (1) a statistical method of experiment in Psychical Research which, it is believed, will be acceptable to science and will prove adequate for resolving doubt and controversy concerning the alleged supernormal acquisition of knowledge (telepathy, lucidity or clairvoyance, or communication from discarnate intelligences capable of apprehending facts in our world); and (2) the results of the first applications of this method.

It will be readily apparent to the scholar that much of the monograph has been written under great pressure, a circumstance regrettable but unavoidable; the work of investigation has not been permitted to suffer interruption, and it was not advisable to delay longer the first report from our laboratory. Haste has not been made at the expense of accuracy, however; and, although the literary quality of the exposition has undoubtedly suffered, it is hoped that the reports of the various researches will be found sufficiently clear and complete to serve their purpose. It should be mentioned, perhaps, that various labor-saving appliances have been utilized, such as calculating and adding machines, mathematical tables, and the slide-rule; the last mentioned having been consistently used in calculating percentages. Mathematical accuracy sufficient for our purpose has certainly been attained. And deficiencies in the plates must be credited to the writer's general ineptitude with India-ink.

The Division of Psychical Research is indebted to Dr. Lillien Jane Martin for PART V, a record of work which she has carried out independent of the Psychical Research Foundation; and also for her zeal in the work of equipping the Psychical Research laboratory.

The writer is under many special obligations to those who have contributed to the investigations, or to the compilation of this monograph: First and foremost to his colleagues in the Department of Psychology for innumerable courtesies with respect not only to sound counsel but also to the free use he has made of laboratory rooms, equipment, and students of their classes; to the many students who have rendered faith-

ful service in the experiments reported herein; to the California Psychical Research Society for its generous coöperation in research in San Francisco; to Professor Milo A. Tucker, and to Mr. G. P. W. Jensen, for indispensable assistance in experiments with 'sensitives' in San Francisco; to Professor E. P. Cubberley, Dean of the School of Education, for the generous loan of a dictaphone for two years; to the Assistant Registrar, J. E. McDowell, for access to students' percentile grades; to the Staff in the Library, particularly to Librarian G. T. Clark, and Miss Lena M. Keller, for assistance in the compilation of the catalogue of works in the psychical-research library; to the American Society for Psychical Research, the *American Journal of Psychology,* and the *Psychological Review,* for permission to use material published in their pages; to students, J. T. Reynolds, D. C. Upp, F. S. Fearing and Miss Else Nagel for faithful clerical and statistical assistance; and to many others for kind offices too numerous for separate mention.

THE FELLOW IN PSYCHICAL RESEARCH.

STANFORD UNIVERSITY,
July 27, 1917.

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INTRODUCTION

BY

PROFESSOR FRANK ANGELL

Psychology has, up to the present, shown no disposition to make its own the problems of psychical research; yet probably no one will be found to deny their importance.—NORTHCOTE W. THOMAS: *Thought-Transference,* London, 1905, p. 20.

Psychical Research is at present in disrepute among scholars, largely because psychical researchers do not take a logical psychological attitude toward the phenomena they investigate. . . . The investigation of phenomena which are alleged to be not in accordance with accepted views of natural law, is a perfectly legitimate activity.—KNIGHT DUNLAP: *A System of Psychology,* 1912, p. 343.

INTRODUCTION

In January of 1912 the writer was informed by Dr. Jordan, then President of the University, that Mr. Thomas Welton Stanford, brother of Leland Stanford and himself one of the University Trustees, had placed at the disposal of the University the sum of £10,000, the interest of which was to be applied to investigations in the field of what may be broadly termed Spiritualism and Psychical Research, and Dr. Jordan asked if the Department of Psychology was willing to assume the grave responsibility of applying the endowment to work in this field.

And here it must be frankly stated that the department felt that any impulsive or hasty acceptance of Mr. Stanford's generous offer was out of place; in justice to both Mr. Stanford and the University the matter was one that called for thoughtful consideration. For it was obvious that the implications inherent in investigations in psychic or spiritualistic phenomena would give the undertaking a different character from that obtaining in ordinary cases of endowments for scientific research. In the first place the problems to be investigated were intimately connected with religious beliefs and opinions of many devout persons, among them Mr. Stanford himself, whose house in Melbourne has long been the home of spiritualistic séances. But tenets of religious faith in St. Paul's sense of "the substance of things hoped for and the evidence of things not seen" are beyond or above or at any rate outside, the methods of scientific investigation; the Department of Psychology is a scientific department of the University and its methods of research must necessarily advance in accordance with the canons of scientific methods, that is, accurate observation and careful verification of accessible phenomena. To subject matters on which good men and true had based comforting and abiding faith to the cold criticism of scientific reason would be, the writer felt, not only a delicate but perhaps a thankless task. In the next place the situation was further complicated in the country at large and especially in California by the presence among the devout Spiritualists of many false teachers who sought to exploit spiritualistic procedure for pecuniary profit with the natural result of injuring and discrediting the cause of Spiritualism and perplexing those who wished to know who were genuine leaders of the faith. The findings of the Seybert Commission of the University of Pennsylvania had also contributed to the same result, especially among the universities. Mr. Henry Seybert, well-known as an

earnest believer in Spiritualism, presented to the University of Pennsylvania a sum of money sufficient to found a chair of philosophy and to defray the expenses of a commission to investigate the "systems of morals, religion, or philosophy . . . and particularly of modern Spiritualism." This commission was composed of ten members, among them Dr. W. Pepper, Provost of the University, Professor G. S. Fullerton, incumbent of the Chair of Philosophy, and Dr. Weir Mitchell, the well-known neurologist. To these were added Mr. T. R. Hazard, described as an uncompromising believer in Spiritualism. The commission investigated all the well-known professional mediums they could induce to come to Philadelphia: their findings were uniformly unfavorable to the pretensions of the mediums and, in most cases, they reported fraud. Among the conditions, consequently, which gave the Department pause in coming to a decision were what Sir W. F. Barrett has termed "the scornful attitude of the scientific world" together with the somewhat delicate nature of Psychical Research on account of religious implications. As far as Mr. Stanford's attitude was concerned it was all the University could wish; the endowment was wholly unconditioned and there were no limits as regards time and no suggestions as regards problems or results. In these respects, therefore, there was no reason why the University should not gladly accept the endowment. In addition the *carte blanche* given by Mr. Stanford freed the department from the feeling that it would be unduly hampered in its investigations by religious complications; it was simply to be a matter of scientific investigation.

The question then arose of whether in view of Professor Sidgwick's authoritative utterance to the effect that Psychical Research so far as he could tell, had made no discernible progress in the last twenty years, the field was not a slough of despond through which no scientific progress was possible. The writer's opinion was that intensive investigation by trained psychologists devoting themselves wholly to this work, beginning with the simpler problems, would bring forth results of scientific value, though manifestly if Sidgwick's view of the impracticable nature of the field was even approximately correct, but slow progress could be expected. However, before coming to any final decision in the matter, letters were sent to the psychology departments of other universities asking their opinion of the probable worth of investigations in this field. The answers were uniformly favorable to the undertaking and from two especially, Cornell University and the University of California, there came valuable suggestions in regard to problems and to methods of investigation.

Feeling then that the work could be taken up in fairness both to the University and to Mr. Stanford the Department of Psychology accepted the responsibility of administering the endowment. The endowment itself was large enough to defray the expenses of a Fellowship, to refit completely and equip the laboratory rooms assigned to the work by the Department and to supplement the apparatus which the Department was able to furnish with special instruments for Psychical Research. In addition Mr. Stanford placed about £100 a year at the disposal of the University for the purchase of books on psychical research and finally added to these donations the large collection of 'apports' produced in the séances at his house in Melbourne.[1]

One of the reasons that may be assigned for the lack of progress in Psychical Research and spiritualistic problems of which Professor Sidgwick complains is in all probability that the greater part of the investigations have been carried on by amateurs rather than by 'professionals,' by those for whom the work was rather an avocation than a special calling. Thus the Seybert Commission, as the report states, was made up of men whose days were "already filled with duties which cannot be laid aside and who are, therefore, able to devote but a small portion of their time to these investigations." This condition is reflected in a great many of the publications on Psychical Research. The writers have taken up the investigation in the spare hours of the day or the spare months of the year, and, considering the complexity and elusiveness of the phenomena involved, it is small wonder that progress has not been more marked. Closely allied with this is another factor which has been of no advantage to Psychical Research, either as regards its advancement or its standing in the eyes of the scientific world, and that is the factor of attributing to amateurs in psychical investigations the like authority which they enjoy in their chosen profession. It must be said with the utmost frankness that the mantle of Sir Oliver Lodge's great reputation as a physicist cannot be stretched to cover his work in Psychical Research and it is doubtful if Sir William Crooke's authority as a chemist has perceptibly swayed the minds of his colleagues in chemistry towards spiritualistic belief. Obviously, what is necessary for the advance of Psychical Research in the eyes of the scientific world is precisely what all other kinds of scientific work demand; that is, the undivided time and attention of investigators possess-

[1] Most of the books purchased with the funds are placed on the shelves of the general library. The 'apports' are kept in 'display' cases in a special room adjoining the laboratory. A plan of the laboratory for Psychical Research will be found at the end of this INTRODUCTION (see Plate I, p. xxiv).

ing a special training for their work. In this field, for example, it would mean special extensive training in the psychology of motor automatisms and of subliminal impressions, in the ideational and affective processes underlying belief and conviction, in illusions of perception and the value of evidence. Through the endowment of Mr. Stanford this university was placed in a position to fulfill these conditions and to realize Sir Oliver Lodge's wishes expressed years ago, for "a laboratory with special appliances." The selection of the incumbent of the fellowship was a matter of no less importance than the facilities for work, and after diligent inquiry into the qualifications of men eligible for the position, the choice was made of Dr. J. E. Coover,—a well-trained and able psychologist and a mature man of highly judicial temperament. To dignify the fellowship in the regard of the university world, the Trustees conferred on Dr. Coover the rank of Assistant Professor. The investigations in this volume made by Dr. Coover, and the vast mass of data gathered by him are an index, or at least a partial index, of his unflagging devotion to the work. I say partial index as the time taken for the investigation of mediums in San Francisco was out of proportion to the amount of data collected. Too frequently these trips were barren of all results, the investigator having spent hours in the dark awaiting manifestations which either wholly failed to appear or appeared but feebly and infrequently.

In selecting problems for investigation the logical postulate of simplicity was given great weight and for this reason "The Feeling of Being Stared At" was the first to be chosen. For a belief in the efficacy of this feeling is wide-spread among the students, it is a subject that admits of easy experimentation, and, what is highly important, it is directly connected with the general problem of telepathy. A further postulate of the work was to shape the early investigations to the material in hand,—in this case the numerous students taking work in psychology. Through them there was given an opportunity for statistical studies in telepathy along the lines laid down by the English Society for Psychical Research, and in addition there was always the chance in dealing with a large number of individuals of discovering someone unusually gifted with telepathic powers.

Other investigations which could be conducted *in situ* were on problems of subliminal activity, in mental habits or bias in forming judgments, and on the implications of spoken words (sound assimilation) all of which form necessary *prolegomena* to the clear understanding of spiritualistic manifestations.

In view of the mass of work in evidence in the pages of this report it will be readily understood that time was lacking to go deeply into the

subject of automatic activities, of automatic writing or speaking. The investigation of 'sensitives' or mediums was taken up after considerable experience in methods and procedure in testing psychical manifestations with students. The writer shares Professor Sir W. F. Barrett's distrust of professional or paid mediums and of working in the dark, but Dr. Coover undertook investigations of this kind upon a guarantee of the good faith of the 'sensitives' by the California Psychical Research Society. It is to be regretted that the 'sensitives' felt unable to come to the University to develop their manifestations where they were best fitted to be tested and although a very cordial *entente* exists between Dr. Coover and the California Psychical Research Society in carrying out his investigations, owing to the frequent indisposition of the 'sensitives,' the findings of this part of the report are more scanty than could be wished.

Somewhere Sir Oliver Lodge has raised the question of the advisability of investigating "that of which we are sure." "Why conduct experiments in hypnotism or telepathy?" to which he answers that "Belief is both the prelude to and the outcome of knowledge" and further "If a fact or a theory has had a *prima facie* case made out for it, subsequent investigation is necessary to examine and defend it."

Now so far as the matters of which Sir Oliver Lodge speaks are accessible to scientific investigation, no one would venture to demur to these statements. But the more intimate matters of religious faith the writer does not feel are accessible to experimentation. As to many phenomena which are often regarded as supernormal, the scientific world has no doubt but that with patient and impartial investigation they will ultimately be brought within the circle of the general laws of Psychology as has been the case with the once baffling phenomena of Hypnotism. But for the deeper-seated convictions of personal religion, scientific investigation is out of place.

In establishing the fellowship for Psychical Research Mr. Stanford has made a substantial contribution toward delimiting the borders of these two regions of human experience, and in the matter presented in this volume the writer feels that a substantial contribution has been made to that side of Psychical Research which is accessible to scientific investigation.

FRANK ANGELL.

STANFORD UNIVERSITY,
June 1, 1917.

KEY TO FURNISHINGS

1. Radiator.
2. Apport Case.
3. Apparatus Case.
4. Apparatus Table.
5. Laboratory Table
6. Electric Switchboard.
7. Bookcase.
8. Blackboard.
9. Fire Hose.
10. Telephone.
11. Sink.
12. Desk (Roll Top).
13. Desk (Typewriter).
14. Chart Case.
15. Filing Cases.
16. Smoke Hood and Flue.
17. Motor Generator.
18. Master Switchboard.
19. Batteries.
20. Table.
21. Drawing Board.
22. Dictaphone

ROOM DIRECTORY

415. Apport Room.
416. Library and Apport Room.
416 1-2. Office.
417. Laboratory.
418. Laboratory.
419. Laboratory.
419a. Chronoscope Room.
419b. Dark Room.
419c. Smoke and Fixing Room.

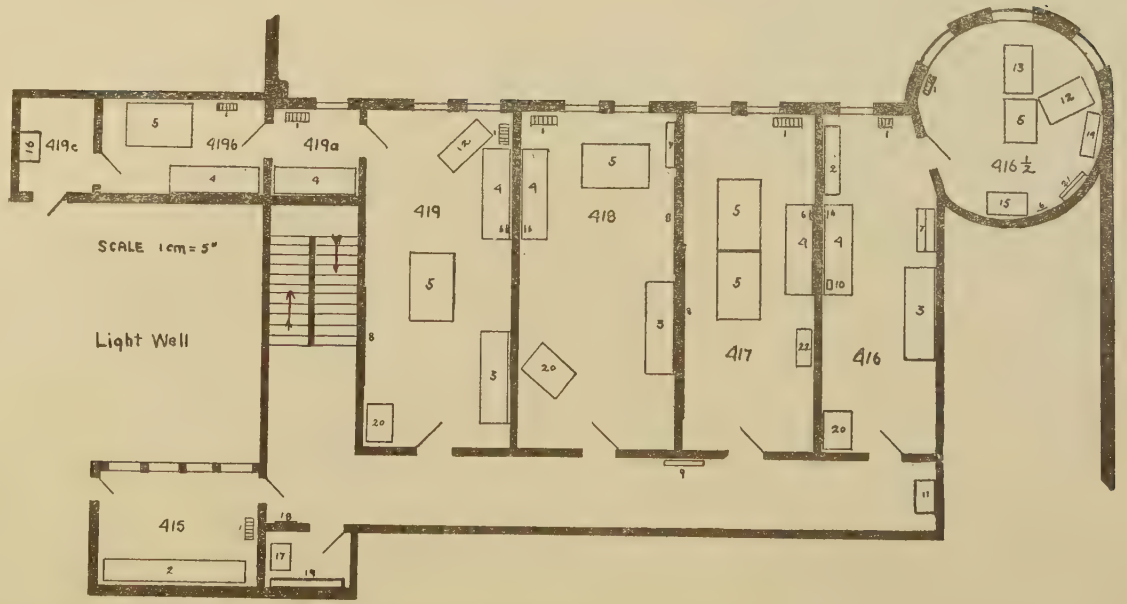

Plate I.—Floor-Plan of the Division of Psychical Research. (The rooms are on the third floor in the Physics-Psychology Building. The Office is now numbered 416a, instead of 416½).

EXPERIMENTS IN PSYCHICAL RESEARCH

PART I.

THOUGHT-TRANSFERENCE.

It is an obvious fact, but it is nevertheless a fact which we must repeat as often as possible, that in no way can psychical research be better aided than by constant and varied experiments on Thought-Transference in every form.—FREDERIC W. H. MYERS: *Proceedings S. P. R.*, 1884, 2:217.

Upon one other interest I have not yet touched—to me the weightiest and the farthest reaching of all.

No incident in my scientific career is more widely known than the part I took many years ago in certain psychic researches. Thirty years have passed since I published an account of experiments tending to show that outside our scientific knowledge there exists a Force exercised by intelligence differing from the ordinary intelligence common to mortals. . . . I think I see a little farther now. . . . And were I now introducing for the first time these inquiries to the world of science I should choose a starting-point different from that of old. It would be well to begin with *telepathy;* with the fundamental law, as I believe it to be, that thoughts and images may be transferred from one mind to another without the agency of the recognized organs of sense—that knowledge may enter the human mind without being communicated in any hitherto known or recognized ways.—SIR WILLIAM CROOKES, in *The Presidential Address, delivered to the British Association for the Advancement of Science,* at Bristol, September 1898, (*Proceedings S. P. R.*, 14:2-3).

EXPERIMENTS IN PSYCHICAL RESEARCH

PART I.

THOUGHT-TRANSFERENCE.

The Present Importance of the Problem.

An examination of the literature of Psychical Research reveals the paramount importance of Telepathy, or Thought-Transference,[1] among all the various kinds of phenomena which fall within its field. Not only have the principal psychical research societies given the investigation of this process a prominent place in their formally announced aims of organization and given it their chief attention during the earlier years of their work, but at the present time, when both the English and the American societies are seeking indisputable evidence for the survival of human personality beyond bodily death, this process threatens to cut to the root of their proof.

The evidence regarded by the leaders in psychical research as the most promising for proof of survival lies in the content of the utterances (spoken, written or signaled) proceeding from an "automatist" or a "psychic," usually entranced. That it cannot be regarded as merely normal phenomena is most positively affirmed by those who have examined it with the greatest care; and some proponents of its extra-normal character are celebrated psychologists, whose professional and critical judgment applies precisely to the normal and abnormal behavior of the mind.

Professor James has several times given voice to his position. As early as 1890 he wrote concerning Mrs. Piper's "messages":

> My later knowledge of her sittings and personal acquaintance with her has led me . . . to believe that she has supernormal powers. (p. 652).

[1] Although these terms are sometimes assigned different meanings, they have not been shown to be different kinds of functions, and for our purpose they may be regarded as synonymous, meaning an influence of one mind upon another otherwise than through the recognized sensory channels; the influence may take the form of a sensation, an idea, a thought, a desire, an emotion, or any other assignable content of consciousness.

And in another paragraph:

Taking everything that I know of Mrs. Piper into account, the result is to make me feel as absolutely certain as I am of any personal fact in the world that she knows things in her trances which she cannot possibly have heard in her waking state. (pp. 658-9).[2]

In 1896:

In the trances of this medium, I cannot resist the conviction that knowledge appears which she has never gained by the ordinary waking use of her eyes and ears and wits. (p. 319).[3]

And in 1909, when summing up his "Final Impressions" after twenty-five years' experience in psychical research, concerning automatic utterances he wrote:

When imposture has been checked off as far as possible, when chance coincidence has been allowed for, when opportunities for normal knowledge on the part of the subject have been noted, and skill in "fishing" and following clues unwittingly furnished by the voice or face of bystanders have been counted in, those who have the fullest acquaintance with the phenomena admit that in good mediums *there is a residuum of knowledge displayed* that can only be called supernormal: the medium taps some source of information not open to ordinary people. Myers used the word "telepathy" to indicate that the sitter's own thoughts or feelings may be thus directly tapped. (pp. 188-9).

I wish to go on record for *the presence,* in the midst of all the humbug, *of really supernormal knowledge,* [with strong mediums]. (p. 200).[4]

Professor Flournoy, Professor of Psychology in the University of Geneva, in 1900, said:

Taking everything into consideration, I am inclined to believe that Mlle. Smith, in truth, possesses real phenomena of clairvoyance, not, however, passing beyond the possible limits of telepathy . . . (p. 397).[5]

Podmore, one of the most conservative writers, and perhaps the most critical student, in the English Society, in 1910, said:

The automatists unquestionably show that they possess information which could not have reached their consciousness by normal means, and it is in tracing this information to its source that the main interest of the inquiry and the main burden of proof will be found. (p. 302).[6]

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[2] James: A record of observations of certain phenomena of trance. *Proceedings S. P. R.,* 1890, 6:651-659.

[3] James: The Will to Believe. New York, 1899.

[4] James: Memories and Studies. New York, 1911.

[5] Flournoy, Th.: From India to the Planet Mars, a study of a case of somnambulism with glossolalia. (tr. Vermilye). New York, c1900.

[6] Podmore, Frank: The Newer Spiritualism. London, 1910.

The Rôle of Telepathy as an Alternative Hypothesis to Spirit Communication.

Sir Oliver Lodge, in his Presidential Address to the English Society for Psychical Research, in 1902, pointed out the alternate hypotheses for "trance lucidity and clairvoyance" as (1) Telepathy from the living, and (2) Communication from the departed;[7] and in 1913 he restated his conviction that telepathy serves as an hypothetical explanation for a multiplicity of phenomena,—"it is the minimum hypothesis."[8]

A classification of the factors involved in trance utterances was offered by Frederick W. H. Myers:[9]

(*a*) Dreamlike and confused talk from the subliminal self,

(*b*) Facts implying the perception of events occurring at a distance—clairvoyance,

(*c*) Facts existing in the minds of the sitters,

(*d*) Facts not known to sitters but which would have been known to the departed persons.

And in the same place he affirms:

> I believe, then, that I have good reason for ascribing many of these messages to definite surviving personalities, known while on earth to friends of mine whose presence with Mrs. Thompson has evoked the messages, or to myself. I believe that most of these messages are uttered through Mrs. Thompson's organism by spirits who for the time inform or "possess" that organism; and that some are received by her spirit in the unseen world, directly from other spirits, and are then partially remembered, so that the sensitive can record them on emerging from the ecstatic state. (p. 73).

The seriousness of telepathy as an alternative hypothesis is stated by N. W. Thomas, Professor Flournoy, and Professor Hyslop, among others. Thomas:

> The evidence for spiritualism . . . suffers . . . so long as it is conceivable, if perhaps improbable, that all the facts on which spiritualists rely, can be explained away by a telepathic hypothesis. (p. 178). . . . The extent of the evidence for spiritualism must remain a matter of doubt, and be liable to reduction in proportion as we can justly ascribe to telepathy the supercognitive phenomena of trance mediumship. (pp. 178-9).[10]

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[7] Lodge, Sir Oliver: President's address. *Proceedings S. P. R.*, 1902, 17:38.

[8] Lodge: Telepathy as a fact of experience. *Bedrock*, 1913, 2:57 ff.

[9] Myers, F. W. H.: On the trance-phenomena of Mrs. Thompson. *Proceedings S. P. R.*, 1902, 17:67-74.

[10] Thomas, Northcote W.: Thought Transference. London, 1905.

Flournoy:

Certain it is that telepathy takes away all evidential value from certain communications received which might otherwise be thought to be spiritistic in character. (p. 209).[11]

Hyslop:

The crucial test of spiritism, in this and all other cases, must turn upon the question of telepathy to furnish the data upon which any secondary consciousness has to work.[12]

This dilemma may be more adequately appreciated by the reader after he gets a closer view of available illustrations in the discussions offered by the leaders in psychical research by way of interpretation of trance and other automatic communications. The alleged super-normal character of these communications may be conceived to vary in a graded series from (1) the simplest case of telepathic reproduction of the sitter's present thought, through (2) similar reproduction of the sitter's forgotten memories or unnoticed sensory impressions, (3) similar reproduction of the experience of some third living person ("telepathie à trois"), and (4) the reproduction of the life memories of deceased persons, to (5) more or less direct communication from persisting and still active discarnate personalities. The representative discussions to be reviewed, although they may not be conveniently grouped in a corresponding series, apply to one or more of the members of the series.

First, The reproduction of the sitter's conscious or subliminal experience:

Professor James says:

"Telepathy" seems fairly established as a fact, though its frequency is still questionable: . . . Our rule of presumption should lead us, then, to deny spirits and to explain the Piper-phenomena by a mixture of fraud, subconscious personation, lucky accident, and telepathy, whenever such an explanation remains possible. (p. 34).

[Yet,] the personation, fishing, guessing, using lucky hits, etc., in Mrs. Piper, may be, as it were, the mechanical means by which "spirits" succeed in making her living organism express their thought, however imperfectly. (p. 35).

I myself can perfectly imagine spirit-agency, and I find my mind vacillating about it curiously. (p. 35).[13]

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[11] Flournoy, Theodore: Spiritism and Psychology. (tr. Carrington). London, 1911.

[12] Hyslop: A further record of observations of certain trance phenomena. *Proceedings S. P. R.,* 1901, 16:292.

[13] James: Report on Mrs. Piper's Hodgson-control. *Proceedings S. P. R.,* 1909, 23: 2-121.

Professor Flournoy says:

All the facts of lucidity (clairvoyance, second sight, etc.) which are attributed to Mlle. Smith may be explained by telepathic impressions proceeding from living persons. (p. 396).[14]

Dr. Hodgson, the skilled researcher, sent by the English S. P. R. to investigate the "phenomena" of Mrs. Piper in America, made a voluminous report in 1892, in which he said:

Putting aside all the facts which can be explained by direct thought-transference from the sitter, and considering simply the information given which was not known to the sitter and which purports to come from "deceased" persons, but which was known to, and afterwards verified by, distant living persons,—is there sufficient ground for concluding that Phinuit is in direct communication with "deceased" persons, and that he is a deceased person himself as he alleges? I think that the evidence here presented, together with that previously published, is very far from sufficient to establish any such conclusion.

[The most satisfactory hypothesis seems to be that in her automatic trance] a secondary personality of Mrs. Piper either erroneously believes itself to be, or consciously and falsely pretends to be, the "spirit" of a deceased human being, Phinuit. (p. 57).[15]

But in a later report, which included evidences of the continued existence of the late "George Pelham," he concluded:

. . . Many of what were once difficulties to myself in the way of believing that these phenomena were the result of the agency of "deceased" persons, have been removed by the fuller evidence presented by [the later control] G. P. and other communicators acting directly. (p. 405). . . . What my future beliefs may be, I do not know. Röntgen suggested that certain special effects produced in his famous experiments were due to rays whose vibrations were longitudinal to the path of propagation, but later experiments have tended to show that they are due to vibrations of the same general character as those with which we were familiar, but of a higher order of frequency. And it may be that further experiment in the lines of investigation before us may lead me to change my view; but at the present time I cannot profess to have any doubt but that the chief "communicators," to whom I have referred in the foregoing pages, are veritably the personalities that they claim to be, that they have survived the change we call death, and that they have directly communicated with us whom we call living, through Mrs. Piper's entranced organism. (pp. 405-6).[16]

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[14] Flournoy, Th.: From India to the Planet Mars, a study of a case of somnambulism with glossolalia. (tr. Vermilye). New York, c1900.

[15] Hodgson, Richard: A record of observations of certain phenomena of trance. *Proceedings S. P. R.*, 1892, 8: 1-167.

[16] Hodgson, Richard: A further record of observations of certain phenomena of trance. *Proceedings S. P. R.*, 1897-8, 13: 284-582.

Concerning later Piper sittings, Sir Oliver Lodge, in 1909, said:

On the whole, they tend to render certain the existence of some outside intelligence or control, distinct from consciousness, and as far as I can judge from the subconsciousness also, of Mrs. Piper or other medium. . . . I feel that we are in secondary or tertiary touch—at least occasionally—with some stratum of the surviving personality of the individuals who are represented as sending messages. (p. 282).[17]

And concerning the Mrs. Grove case, he wrote:

This series, for several reasons, must be regarded as the most strictly *evidential* of all; and a decided unity of character and of message is preserved, no matter through what medium the communication comes. But the hypothesis of telepathy from the sitter, if stretched sufficiently, will cover all the reported portions; and in such a case this notion constitutes a difficulty which can hardly be avoided. At the same time I must say that I find this hypothesis not very probable,—it does not at all satisfy my mind as an explanation. On the whole, the surviving and communicating intelligence hypothesis commends itself to me as the most likely. (p. 283).[17]

Hyslop, in his report on his study of the "phenomena" of Mrs. Piper for evidences of spirit identity, said:

The evidence for personal identity in this record is so overwhelming, that when we dismiss fraud from consideration and reckon the mistakes and confusions in the favor of spiritism instead of difficulties and objections, we should not naturally suspect telepathy as the most probable hypothesis in the case. The specter which that doctrine raises is of the Society's own making in phenomena wholly outside the field I am considering here, and obtains its cogency far more from our mental habits than from the facts of this record. (p. 242).[18]

Podmore, in 1903, wrote:

Prior to the publication, in 1898, of Dr. Hodgson's monumental report on Mrs. Piper's later trances (*Proceedings S.P.R.*, vol. 13), I had held that her utterances were amongst the strongest evidences which we possessed for telepathy, or at least for some supernormal faculty of acquiring information outside the possible radius of the senses; on the other hand, it seemed to me that the indications of the action of discarnate spirits were so slight and shadowy as to be hardly worth taking into account. After some conversations with Dr. Hodgson during his visit to this country in 1897, and careful study of the *Report* issued shortly afterwards, I inclined to the opinion that the case of spirit intercourse was at any rate strong enough to be accepted as a provisional hypothesis. . . . The effect of . . .

[17] Lodge, Sir Oliver: Report on some trance communications received through Mrs. Piper. *Proceedings S. P. R.*, 23: 127-285.

[18] Hyslop, James H.: A further record of observations of certain trance phenomena. *Proceedings S. P. R.*, 1901, 16: 4-648.

[Professor Hyslop's] report on my mind has been not merely to discredit altogether the spirit hypothesis so far as this particular series of séances is concerned, but restrospectively to cast some shadow of doubt on the results previously recorded by Dr. Hodgson. (p. 375).[19]

In 1908:

On the one hand, it seems clear that the trance consciousness of Mrs. Piper, as of all other so-called mediums, is apt on very small provocation to personify itself, and that the personification may be shaped by the suggestions of those present. In Mrs. Piper's case we have ground for assuming that such suggestions may often be conveyed telepathically; in short, that the dramatic personalities of the so-called controls may actually be built up out of the material unconsciously supplied by the sitters, and that the intimate personal details revealed in the trance utterances may be telepathically filched from the same source. The limitations of the knowledge displayed, and the occasional disingenuousness, forbid us to accept these communications as authentic and unembarrassed messages from the dead. (p. 329).[20]

And in 1910, concerning the phenomena of the same psychic he wrote:

The change in the character of the recent sittings and the remarkable and life-like development of some of the trance personalities is, no doubt, consistent with the hypothesis of spirit control. But it would not be safe to build much upon such an argument. . . . The only test that we can apply to these earlier sittings lies in the substance of the communications themselves. The great bulk of the information given was, of course, within the knowledge of the sitter, and, apart from its dramatic form, there is no ground for assuming any other source than telepathy from his mind. (p. 305).[21]

Flournoy wrote:

We might say that telepathy between the living—particularly between the medium and members of a spiritistic group—is one of these laws, although still vague as to its necessary conditions. The only point which appears to me worthy of being raised, because it is so often observed, is that the ideas of the sitters which have the greatest chance of being transmitted to the medium are those in a sort of nascent or evanescent condition, upon the threshold between consciousness and subconsciousness, and passing from one to the other. (pp. 211-212).

Many people going to consult a medium are astonished that the medium tells them nothing that they are thinking about, but reveals to them details of which they did not dream. . . . The psychic processes about to blossom or to fade away in the penumbra of consciousness have more power of radiating to other

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[19] Podmore, Frank: On Professor Hyslop's Report on his sittings with Mrs. Piper. *Proceeedings S. P. R.*, 1903, 17:374-388.

[20] Podmore: The Naturalization of the Supernatural. New York, 1908.

[21] Podmore: The Newer Spiritualism. London, 1910.

brains than those which are partly immovable—either in the foreground of attention or in the lowest stratum of the subconsciousness. (p. 212).[22]

This principle of direct communication between minds is curiously extended to unsuspected lengths, so firmly has it taken hold of the public mind: Maeterlinck[23] attributed the success of the Elberfeld horses in their performances alleged to express a high degree of intelligence, to subliminal telepathic transference of the answers to problems from the human to the equine mind. F. C. S. Schiller remarking this, says:

To test the telepathic hypothesis he tried a number of experiments, of which the answers were not known to him or any one present, and found that the horses answered as correctly as when the answers were known. . . . [He] betakes himself to the suggestion that some animals, *e. g.*, horses, dogs, and cats (but not elephants and monkeys), are natural "psychics," and so can tap subliminally what Professor James called a great "cosmic reservoir," in which all knowledge is conserved. . . . It will be interesting, however, to see whether experimental confirmation of this mystical hypothesis can be obtained, and also whether any of the many philosophers who profess to hold it on theoretic grounds will take any steps to verify it practically.[24]

Second, The reproduction of the experience of some third living son (telepathie à trois).

Andrew Lang, in 1900, wrote:

I see no reason for the hypothesis that Mrs. Piper ever receives telepathic communications from the dead. Has she ever communicated a single thing that was known to a dead person, but to no living man or woman? Such are my doubts. (p. 52).[25]

And Frank Podmore, in 1910:

The analysis of these cases where information unknown to the sitter was given by the trance intelligence scarcely adds strength to the hypothesis of spirit communication. In every case the information given was, or may have been, within the knowledge of some living mind. In many cases all the circumstances point to some form of telepathy between the distant agent and the trance intelligence, mediated, as it would seem in all cases, by the presence of a common acquaintance in the person of the sitter. (p. 311).

The trance personalities, then, have never told us anything which was not

[22] Flournoy, Theodore: Spiritism and Psychology. (tr. Carrington). London, 1911.

[23] *Neue Rundschau,* June 1914.

[24] *Journal S. P. R.,* July 1914, 16:271-2.

[25] Lang, Andrew: Discussion of the trance phenomena of Mrs. Piper. III.—Reflections on Mrs. Piper and telepathy. *Proceedings S. P. R.,* 1900-01, 15:39-52.

possibly, scarcely anything which was not probably, within the knowledge of some living person. (p. 312).[26]

Third, The reproduction of the same or similar ideas through two or more independent automatists, purporting to be communications from the same discarnate personality, (cross-correspondences, simple).

Miss Alice Johnson observes:

If we simply find the same idea expressed—even in different forms—by both [independent automatists] . . . it may . . . most easily be explained by telepathy between them. (p. 375).[27]

And Professor A. C. Pigou suggests [28] that telepathic capacity of the order illustrated by the experiments of the Misses Miles and Ramsden [29] (telepathy at a distance) is adequate to explain the single correspondences in the communications of independent psychics.

Fourth, The reproduction through two or more independent automatists of ideas the relation of which is known only to the communicating intelligence, presumably a discarnate personality, and can be found by the researchers only after painstaking study and search (cross-correspondences, complementary).

Miss Johnson describes this phenomenon:

What we get is a fragmentary utterance in one script, which seems to have no particular point or meaning, and another fragmentary utterance in the other, of an equally pointless character: but when we put the two together, we see that they supplement one another, and that there is apparently one coherent idea underlying both, but only partially expressed in each. . . . It is . . . difficult to suppose that the telepathic perception of *one* fragment could lead to the production of *another* fragment which can only, after careful comparison, be seen to be related to the first. (p. 375).

[The corresponding statements relate] to events in the present which, to all intents and purposes, are unknown to any living person, since the meaning and point of her script is often uncomprehended by each automatist until the solution is found through putting the two scripts together. (p. 377).

It was not the automatists that detected [this new experimental procedure] . . . but a student of the scripts; it has every appearance of being an element imported from outside; it suggests an independent invention, an active intelligence constantly at work in the present, not a mere echo or remnant of individualities of the past. (p. 377).

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[26] Podmore, Frank: The Newer Spiritualism. London, 1910.

[27] Johnson, Alice: On the automatic writing of Mrs. Holland. *Proceedings S. P. R.,* 1909, 21:166-391.

[28] *Proceedings S. P. R.,* 23:292-3. [29] *Proceedings S. P. R.,* 21:60-93.

Perhaps the most serious practical objection to the hypothesis that the controls invented this special plan of cross-correspondences would be that it might have been a subliminal invention of Mrs. Verrall's, since it is on her script that the hypothesis is chiefly based, and it is there that we find the most complete exposition of it. There are, however, a few indications of it in Mrs. Holland's script also, quite independently of Mrs. Verrall's. (p. 389).[30]

Professor Pigou comments:

All the characteristics of the best cross-correspondences seem to me to be produced in this experiment. Since, therefore, there are strong grounds for believing that the agent here was the subliminal consciousness of Mr. and Mrs. Verrall, or of both together, there are also strong grounds for believing that the manufacture of cross-correspondences of the required type falls within the compass of incarnate mind. In view of this fact I conclude that the occurrence of these correspondences in other cases does not make probable the operation of any discarnate mind. (p. 302).[31]

Miss Johnson again says:

Are we to suppose, then, that the unconscious and involuntary telepathic efforts of Mrs. Verrall and of Mr. Piddington, acting unconsciously, involuntarily, and telepathically in combination with each other, produced the whole cross-correspondence? (p. 255).

. . . There are, indeed, two or three items which some of the automatists may be supposed to have borrowed telepathically from one another. . . . But looking at the scripts as a whole, we find an extraordinary variety in the methods chosen to approach the same idea. . . . (p. 256). It appears to me, in short, that many of the items of this cross-correspondence afford strong evidence of the design or agency of some intelligence which was *cognisant of the whole scheme,* as finally revealed . . . and it seems to me difficult to attribute so complete a knowledge of it to the subliminal consciousness either of Mrs. Verrall or Mr. Piddington. (p. 261).[32]

And after still further study, Miss Johnson observes:

I would next point out that, while the cross-correspondences between the scripts of different writers seemed at first to consist merely of verbal similarities or coincidences of topic, further study showed that they were far more complicated and elaborate than we had supposed, involving many more scripts and often several different subjects; sometimes including items of literary or historical interest unknown to the writers whose script furnished the cross-correspondence

[30] Johnson, Alice: On the automatic writing of Mrs. Holland. *Proceedings S. P. R.,* 1909, 21:166-391.

[31] Pigou, Professor A. C.: Psychical research and survival after bodily death. *Proceedings S. P. R.,* 1909, 23:286-303.

[32] Johnson, Alice: Second report on Mrs. Holland's script. *Proceedings S. P. R.,* 1910, 24:201-263.

and characteristic in many ways of the supposed "author," and in general more and more difficult to explain on the hypothesis of the unaided subliminal powers of the writers. (p. 155).[33]

Podmore writes:

No person who carefully studies the records would think it possible to attribute all these numerous and well-attested coincidences to fraudulent design or the mere chance association of ideas. If we reject, for the present, at any rate, the explanation suggested by many of the utterances themselves, that of communication from the dead, we must seek for some other cause adequate to the effects. There remains only the agency which has been provisionally named telepathy. . . . The establishment of such a faculty, if only as the vestige of a primitive mode of sensibility, now superseded by articulate speech, would surely be a result worth all the labor spent [in this field]. (p. 316).

But whatever the explanation of this particular series of coincidences [Verrall-Frith-Holland-Piper "The Sevens Incident"] I see no evidence whatever to justify the assumption, even provisionally, of a directing intelligence other than those of the automatists concerned. It would appear, on the contrary, that this case has important bearings upon the interpretation of the evidence as a whole. Not only does it vindicate, in the least equivocal fashion, the action of telepathy from the living, but it further invalidates by anticipation all the evidence for the agency of the dead which might have been derived from "posthumous" letters, and has thus deprived us of what would have seemed an important, though not, of course, a crucial, test. (p. 276).

In fact, the investigators themselves now recognize that the primitive theory of possession, the theory advocated in a modified form by Dr. Hodgson and still held by most Spiritualists, can no longer be defended. They have substituted for it a theory of telepathic interaction between the mind of the automatist and other minds, of the living or of the dead. (p. 298).[34]

This Dominating Rôle of Telepathy Challenged.

But this dominating rôle of the hypothesis of telepathy in the explanation of trance utterances has not been uncontested, and that from quite diverse quarters:

First, Telepathie à trois, it is claimed, has not been proved. Lodge, in 1902, said:

Returning to the subject of trance-lucidity generally, I wish to emphasize my conviction that an explanation based on telepathy as a *vera causa* can be pressed too far. Telepathy is the one ultra-normal human faculty to the reality of which most of those who have engaged in these researches are prepared to assent: that is, to assent to it as a bare fact, a summary of certain observed phe-

[33] Johnson, Miss Alice: A reconstruction of some "Concordant Automatisms." *Proceedings S. P. R.*, 1914, 27:1-156.

[34] Podmore, Frank: The Newer Spiritualism. London, 1910.

nomena; but its laws are unknown and its scope and meaning are not yet apparent. (p. 39). Until we can answer these questions . . . it is scarcely possible to regard telepathy, even from the sitter, as a legitimate explanation of much of the clairvoyance or lucidity noticed in trance utterances. It may have to be assumed as the least strained explanation, but it cannot with certainty be definitely asserted to be the correct one, even when it would easily cover the facts; still less is it permissible, except as the vaguest and most groping hypothesis, to press it whenever convenient beyond the limits of experiment into an extrapolated region, and to suppose that the minds of entirely disconnected and unconscious strangers at a distance are actually read: when it has never been experimentally shown that they *can* be read at all. (p. 39).[35]

Second, Telepathy, as supported by "spontaneous cases" (as, apparitions of the living or the dying), is not proved.

Podmore wrote, in 1894:

The kind of evidence now to be considered the coincidence of some spontaneous affection of the percipient with some event in the life-history of the person presumed to be the agent, as when one sees the apparition of a friend at the time of his death—is of inferior cogency in two ways [to the evidence from experimental cases]: the coincidences are neither so numerous nor so exact; and the risk of error in the record is far greater. (p. 143).[36]

And in 1910:

It is hardly necessary to say that these faculties (telepathy, clairvoyance, precognition, and the like) have not yet been admitted to the rights of citizenship in the republic of science, though one of them (telepathy) has filed a petition for naturalization. Let us examine first the claims of this aspirant, telepathy. . . . Now, the main evidence for the operation of this presumed faculty of telepathy consists of experiments in which the two parties to the transfer, the agent and the percipient, were in the same room, or, at any rate, within a few yards of each other. Many series of successful experiments in the transference of ideas and sensations have been conducted under these conditions. . . . But it was found that a slight increase in the distance exercised a marked effect on the result. . . . There have been a few isolated instances, and a few short series of experiments, in which it is claimed that definite ideas of numbers, objects, or pictures have been telepathically transferred between agent and percipient when separated by distances varying from hundreds of yards to hundreds of miles. But when we remember the habitual inaccuracy of untrained investigators, and the various sources of error in experiments of this kind, together with the practical certainty that the successes reported, even if recorded with perfect accuracy, bear but an infinitesimal proportion to the unrecorded failures, it is impossible to assign much

[35] Lodge, Sir Oliver: President's address. *Proceedings S.P.R.*, 1902, 17: 37-57.

[36] Podmore: Apparitions and Thought-Transference, an examination of the evidence for Telepathy. London, 1894.

weight to these sporadic instances of "thought transference at a distance." (pp. 21-23).[37]

We can hardly be justified in making the spontaneous phenomena [apparitions at the time of death] the basis of a theory of telepathy. (p. 26). . . . Considered by themselves, they hardly carry weight enough to count; it is only because of their presumed kinship with the manifestations of experimental telepathy that they have any claim to be heard at all. (p. 26).[37]

Carrington, 1908:

Without experimental evidence, we should certainly be unwarranted in inventing that theory [telepathy] to explain the spontaneous cases. (p. 198).[38]

Tuckett, 1911:

The evidence for telepathy is at first sight most striking and abundant, so that a belief in its reality is now almost universal, at any rate outside strictly scientific circles. (p. 107). . . . Scientifically all one can say is that the evidence for telepathy is wonderfully suggestive. (p. 109).[39]

And James, 1909:

The peculiarity of the case is just that there are so many sources of possible deception in most of the observations that the whole lot of them *may* be worthless. (p. 175).[40]

Third, Telepathy as supported by direct experiment is not *proved.*

Hyslop, in his introduction to one of Carrington's books, published in 1908, says:

Mr. Carrington brings out clearly that it [telepathy] is not to be regarded as an explanation of anything, and is only a name for facts requiring such an explanation. This is of all things one of the most important qualifications with which the term is to be used. Moreover, it is well to keep in mind that, even as an alleged fact of the supernormal kind, it is not a generally accepted phenomenon in the scientific world. Only a few men seriously believe in it, and others are willing to speak and think of it tolerantly in order to escape a profounder alternative. (pp. 11-12).[41]

James, in 1896, wrote:

No mere reader [of the experimental results] can be blamed, however, if

[37] Podmore, Frank: The Newer Spiritualism. London, 1910.

[38] Carrington, Hereward: The Coming Science. Boston, 1908.

[39] Tuckett, Ivor Ll.: The Evidence for the Supernatural, a critical study made with "uncommon sense." London, 1911.

[40] James: Memories and Studies. New York, 1911.

[41] Carrington, Hereward: The Coming Science. Boston, 1908.

he demand, for so revolutionary a belief, a more overwhelming bulk of testimony than has yet been supplied. (p. 309).[42]

Jastrow, in 1900:

That there is something in these results to be explained is admitted: whether the results have been obtained and recorded in such a way as to contain the clue to their explanation cannot be affirmed; whether our present state of knowledge enables us to explain them may be argued pro and con; whether they are worth serious attention is also a debatable question; but none of these conditions warrants a resort to the telepathic hypothesis. (pp. 98-9).[43]

What is the logical conclusion to be drawn from the data offerable in evidence of some supersensory form of thought-transference . . . ? . . . I can say no more in dismissing the topic than that to me the phenomena represent a complex conglomerate, in which imperfectly recognized modes of sense-action, hyperæsthesia and hysteria, fraud, conscious and unconscious, chance, collusion, similarity of mental processes, an expectant interest in presentiments and a belief in their significance, nervousness and ill health, illusions of memory, hallucinations, suggestion, contagion, and other elements enter into the composition; while defective observation, falsification of memory, forgetfulness of details, bias and prepossession, suggestion from others, lack of training and of a proper investigative temperament, further invalidate and confuse the records of what is supposed to have been observed. Many of the reported facts are not facts at all; others are too distortedly and too deficiently reported to be either intelligible or suggestive; some are accurately observed and properly recorded, and these sometimes contain a probable suggestion of their natural explanation, sometimes must be put down as chance, and more often must be left unexplained. To call this absence of an explanation telepathy is surely no advance; to pose this hypothetic process as the *modus operandi* of any result that can be even remotely and contingently otherwise accounted for seems superfluous; to actually use this hypothesis to account for still more obscure and more indefinite and less clearly established phenomena is a most egregious logical sin. (pp. 103-4).[43]

And N. W. Thomas, in 1905:

The statistical method . . . has been applied to experimental thought-transference data, but it has hardly been recognized that the few complete series which have been published are insufficient even to demonstrate the mere fact of telepathy . . . It would be well for the Society for Psychic Research to recognize this and organize further experiments on a large scale before assuming, as its members commonly do in discussions on trance mediumship, that telepathy is a *vera causa,* and not only needs no further demonstration, but may be invoked on any and every occasion, regardless of the fact that, in so doing, a *rôle* is frequently assigned to it which may well stagger the imagination, though no evidence, scientific or otherwise, has ever been presented for the telepathic power in the extreme

[42] James: The Will to Believe. New York, 1899.

[43] Jastrow, Jos.: Fact and Fable in Psychology. Boston, 1900.

form in which it is invoked, to explain away experiments more readily explained on a spiritistic hypothesis. (pp. 177-8).[44]

The Present Status of Telepathy.

The hypothesis of telepathy, then, is a serious competitor with the spiritistic hypothesis in the explanation of trance and other automatic utterances, and is seriously employed by the foremost psychical researchers. This important rôle, however, is challenged by equally prominent researchers and students of psychical phenomena, challenged in every phase of its alleged operation from its more complex to its simplest forms.

It might be well, therefore, to inquire what status the telepathic hypothesis occupies in the minds of the foremost psychical researchers and of such psychologists as give it attention. Let it be recalled that one of the principal aims of the Society for Psychical Research at the time of its founding, in 1882, was the investigation of thought-transference, and that within ten years the Society presented the great bulk of its evidence in its voluminous *Proceedings.* Some of the leading researchers who had charge of the investigation had already been convinced by experimental evidence, of the fact of telepathy, before this organized effort to produce scientific proof was begun; and others became convinced in the course of investigation.

It will be in the interest of economy if in our canvass of opinion we select more recent statements, and display more freely the more critical but not the extreme opinions.[45]

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[44] Thomas, Northcote W.: Thought Transference. London, 1905.

[45] The reader is referred for systematic reviews of the evidence to the following works:

Sir Oliver Lodge: The Survival of Man. New York: Moffat, 1909.

Professor W. F. Barrett: Psychical Research. London, 1911.

Frank Podmore: Apparitions and Thought-Transference. London: Scott, 1894. The Naturalization of the Supernatural. New York: Putnams, 1908. Telepathic Hallucinations. Halifax: Milner, n. d.

N. W. Thomas: Thought Transference. London, 1905.

Ivor Ll. Tuckett: The Evidence for the Supernatural. London: Paul, 1911.

James H. Hyslop: Psychical Research and the Resurrection. Boston: Small, 1908.

G. Stanley Hall: *Am. Jr. Psychology,* 1888, 1:128 ff.; 1895, 7:135 ff.

For recent controversy, see articles by Tuckett, Lodge, Hill, Lankester, "A Business Man," Armstrong, and "The Hermit of Prague," in *Bedrock, A Quarterly Review of Scientific Thought.* London, vols. 1 and 2 (1912-14).

For Baldwin's Dictionary of Philosophy and Psychology, Mrs. Sidgwick, in 1902, wrote:

The existence of telepathy is not yet generally admitted by the scientific world, but it has been one of the main functions of the Society for Psychical Research to obtain and investigate evidence on the subject. (p. 668).[46]

Podmore, in 1894:

The possibility of the transference of ideas and sensations must be held to be proved by the experiments recorded. That proof can be impugned only on the ground that the precautions taken against communication between agent and percipient by normal means were insufficient. (p. 143).[47]

In 1902:

For my own part, I see no reason to doubt that if the existence of thought-transference should eventually be demonstrated—and I do not claim that the demonstration is or ought to be considered complete—the explanation will be found strictly within the region of natural law. . . . It must be admitted that the older evidence is far more demonstrative. Possibly, apart from two recent items —the experiments at Brighton conducted by Professor and Mrs. Sidgwick and the records of Mrs. Piper's trance-utterances—the question of the reality of such a faculty would hardly seem worth discussion. (Int., p. xvii).[48]

And in 1910:

The evidence for thought-transference at close quarters is experimental; and the experiments have been conducted by such competent investigators as Mrs. Sidgwick, Edmund Gurney, Professor W. F. Barrett, Sir Oliver Lodge, Pierre Janet, Charles Richet, and others of like caliber. Yet, even so, it is still a claimant for scientific recognition. (p. 25).[49]

Constable, in 1911:

I agree with Podmore that experimental cases constitute the strongest evidence we have towards proof of the fact of telepathy, and that a full consideration of the cases leads, practically, to proof of the fact. But I think such cases are more open to suspicion of good faith than spontaneous cases. . . . The very strength, the completeness of the evidence, may point to fraud. (p. 221).[50]

Tuckett, in 1911:

As regards experimental cases of telepathy, I have never yet seen any evidence such as will satisfy a scientific standard of truth, though some of the

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[46] Sidgwick, Mrs. Henry: Psychical Research. Baldwin's Dict. of Phil. and Psychol., 1902.

[47] Podmore: Apparitions and Thought-Transference, an examination of the evidence for Telepathy. London, 1894.

[48] Podmore: Modern Spiritualism, a history and a criticism. New York, 1902.

[49] Podmore: The Newer Spiritualism. London, 1910.

[50] Constable, F. C.: Personality and Telepathy. London, 1911.

results are distinctly striking. Yet in experiments carried out for this purpose such a proof should be possible. (p. 127).[51]

N. W. Thomas, in 1905, concerning his own experiments, said:

The results of the card experiments . . . are hardly sufficiently decisive for it to be possible to base any conclusion on them. (p. 175).[52]

And concerning the status of the telepathic hypothesis:

If I venture to express my own conviction on the subject, it is that much more effort, and, in particular, much more systematic effort, is needed before we can safely assert that telepathy is a proved[53] fact. (p. 176). When we have arrived at that point our task is only begun. No inquiry can lay claim to be scientific which expresses its results in general terms when it can give them in precise terms. . . . Psychical Research must . . . be made a question of statistics if further conclusions are to be based on the results. (pp. 176-7).

Perhaps it may never be possible to formulate a telepathic law in terms like those of the law of gravitation, or to devise such experimental conditions as will enable the student of trance mediums to say with confidence that his results cannot be explained by telepathy. But, until the effort has been made, no investigation into trance mediumship has the data which can alone enable it to formulate reliable conclusions. (pp. 179-80).[54]

Bramwell, who was a member of a committee of the Society for Psychical Research devoted mainly to telepathic experiments, in 1906, wrote:

During the last twenty years I have searched for evidence of telepathy, and also taken part in the experiments of other observers; the results, however, have invariably been negative. (p. 136).

[Referring to the experiments made by eminent men:] Altho their experiments were carefully conducted, it is doubtful whether all possible sources of error were excluded; and I am unable to accept them as conclusive. (p. 142).

After many years' hypnotic work, and frequent opportunities of investigating the experiments of others, I have seen nothing, absolutely nothing, which might be fairly considered as affording even the slightest evidence for the existence of telepathy, or any of the so-called "occult" phenomena. (p. 142).

Despite all this it would be unphilosophic to deny the possibility of telepathy. (p. 143).[55]

Moll, who witnessed some of the experiments conducted by Mrs.

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[51] Tuckett, Ivor Ll.: The Evidence for the Supernatural, a critical study made with "uncommon sense." London, 1911.

[52] Thomas, Northcote W.: Thought Transference. London, 1905.

[53] Proved, the author means, by direct experiment.

[54] Thomas, Northcote W.: Thought Transference. London, 1905.

[55] Bramwell, J. Milne: Hypnotism, its history, practice and theory. London, 1906.

Sidgwick at Brighton, a series already mentioned as noteworthy, in 1909, said:

The experiments were not conclusive. Also the experiments made by the other persons I have mentioned do not stand serious criticism. My own experiments, especially those I made some years ago in conjunction with Max Dessoir, only gave negative results when the necessary precautions were taken. Still, I agree with Loewenfeld that we cannot deny the possibility of there being such a thing as telepathy, or at least the possibility of there being ways of influencing others about which we know nothing in the present day. But up to the present [1909, 4th ed.] no proof of this has been forthcoming. (p. 515).

As I have already mentioned, we need not attach much importance to the fact that a few savants uphold the reality of occultistic phenomena. I myself formerly attached a certain amount of importance to this fact. But since I have observed the utter helplessness of savants directly they enter on methods of investigation with which they are not thoroughly acquainted, I have become convinced that mediums easily lead great savants by the nose. (p. 551).

When I come to look through the vast literature of occultism, I find that I am totally unable to discover even one single series of experiments that carries with it a convincing proof of the reality of occultistic phenomena; nothing but casual observations of unchecked experiments. There was a time when some of the telepathic experiments carried out in England—more especially those made by Guthrie and Birchall—appeared to me, relatively speaking, free from error. Nevertheless, when I take into consideration the way in which the reports are drawn up, I am compelled to admit that those experiments are not convincing. (p. 552).[56]

Simon Newcomb, the American astronomer, and the first President of the American Society for Psychical Research (founded in 1884), in a statement of his impressions after an experience of fifty years with psychical phenomena, said:

Nothing has been brought out by the researches of the [English] Psychical Society and its able collaborators except what we should expect to find in the ordinary course of nature. (p. 139).[57]

Among the psychologists, Flournoy[58] of Geneva and M'Dougall[59] of England, accept the hypothesis. James, already quoted, holds it as a possible alternative to his suggested theory of a "cosmic consciousness" in the explanation of Mrs. Piper's trance phenomena.

Jastrow, a member of the council in the old American Society for

[56] Moll, Albert: Hypnotism, including a study of the chief points of psychotherapeutics and occultism. London, 1909.

[57] Newcomb, Simon: Modern occultism. *Nineteenth Century and After*, January 1909, 65: 126-139.

[58] Flournoy: Spiritism and Psychology, pp. 209 ff.

[59] M'Dougall, William: Body and Mind, London, 1913. p. 349.

Psychical Research, and now for many years in charge of the Department of Psychology at the University of Wisconsin, also quoted above, in 1900, said:

> I regard the acceptance of telepathy as an established phenomenon, as absolutely unwarranted and most unfortunate. (p. 457).[60]

G. Stanley Hall, one of the leading spirits in the founding of the old American Society for Psychical Research (1884), and for some time a vice-president and a member of the council, founder of the first laboratory in America for experimental psychology, at Johns Hopkins University, and now for many years the president of Clark University, was most sympathetic with the aims and purposes of the English S. P. R. at the time of its founding and has followed its work with keen attention. He said, in 1887:

> We have spent much time and labor in repeating with many subjects nearly all the experiments of the English Society, only to find in very many cases an unaccountable proportion of error.[61]

In 1895:

> The writer has diligently read the experiments of the *Proceedings*, and can honestly say that there is not one in which the conditions as reported seem to him satisfactory. . . . Give us one little fact, ever so little, that we can freely test and reproduce once a year in our laboratory. We will cross seas to see it, will acknowledge our mistaken skepticism, and confess telepathy, and turn the research of one laboratory at least in a new direction.[62]

And in 1910:

> Even telepathy seems to me a striking case of the subjection of the intellect by the will-to-believe. . . . Here I have for years had a standard series of tests often tried on believers in telepathy and clairvoyance, but never with a glimmer of success. Only when conditions can be so controlled that, *e. g.*, a teacher can announce beforehand that, on such a day, hour, and place he will demonstrate these things, can or will they be accepted by any sound scientific mind. (xxxi-xxxii).[63]

Pfungst, the clever investigator who published the remarkable report of experiments made with Clever Hans, the celebrated "educated" horse, in 1911, said:

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[60] Jastrow: The modern occult. *Popular Science Monthly*, 1900, vol. 57, foot-note.

[61] *Am. Jr. Psychology*, 1887, 1:143.

[62] *Ibid.*, 1895-96, 7:139.

[63] Tanner, Amy E.: Studies in Spiritism. Introduction by G. Stanley Hall. New York, 1910.

It may be that these truly microscopic movements also play some part in bringing about the success of some of the experiments in telepathy, so-called, (transference of thought from one person to another, ostensibly without any mediation of the senses known to us). In spite of the huge mass of "experimental evidence" which has been collected, chiefly in England and in America, it appears to me that telepathy is nothing but an unproven hypothesis based upon experimental errors. (p. 108, note).[64]

Professor James R. Angell, head of the Department of Psychology at the University of Chicago, in 1912, said:

However, telepathy is not to be laughed out of court merely by ridicule. There is a very respectable body of evidence tending to show that, occasionally at least, such transfer of knowledge has occurred. [How it comes about is not known; moreover,] the rank and file of scientific psychologists probably disbelieve vigorously in the reality of anything except occasional coincidences, such as are met with in all aspects of nature. (p. 147).[65]

And Professor Titchener, head of the Department of Psychology at Cornell University, in 1898, said:

No scientifically-minded psychologist believes in telepathy. [66]

Tuckett, recognized by the leaders in psychical research as a hostile critic, recently (in 1913) summed up the situation as follows:

All the evidence for "spirit-control," "telepathy," and "psychic force," has been obtained under conditions precluding the possibility of being certain that it is not vitiated by fallacies due to fraud, self-deception, or incompleteness of data.[67]

This position is consistent with that maintained by the author in his book,[68] published two years earlier, of which Jastrow in an appreciative review said:

It shows so clearly the necessity of trained judgment, and the saturation of the inquiring mind with a saving grace of logical rectitude, sustained in turn by psychological insight, for a safe conduct through the tangled thicket from which so many a traveler returns with strange tales and stranger beliefs. (p. 461).[69]

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[64] Pfungst, Oscar: Clever Hans, the Horse of Mr. von Osten; a contribution to experimental, animal and human psychology. New York, 1911.

[65] Angell, James Roland: Chapters from Modern Psychology. New York, 1912.

[66] Titchener: The feeling of being stared at. *Science,* 1898, 8:896.

[67] Tuckett: Psychical Research: The illogical position of some psychical researchers; A rejoinder to Sir Oliver Lodge and Mr. Hill. *Bedrock,* 1913, 1:470.

[68] Tuckett: The Evidence for the Supernatural: a critical study made with "uncommon sense." London, 1911.

[69] Jastrow: *The Dial,* 1912, 52:461-3.

Further Experimental Work Imperative.

That the criticisms leveled at the evidence upon which the leaders of psychical research have erected the telepathic hypothesis, now exploited everywhere in the explanation of psychical phenomena, have not been without effect upon the leaders themselves, is indicated by a perhaps unanimous agreement that the old successful experimental results need further verification, under such conditions of experiment as will both eliminate sources of error prejudicial to the establishing of the fact of telepathy, and reveal somewhat the nature of the process, *e. g.*, whether it follows the laws of radiant energy, or whether it is "non-material." Proof acceptable to science is desired.

Thus, Lodge is still engaged in experiments on telepathy, and the Society officially issued a recent call (February 1915) to its members and associates for assistance:

> *To Members and Associates of the Society for Psychical Research*:
>
> The Society for Psychical Research is anxious to try experiments of various kinds, hypnotic and other, with a view to obtaining further evidence either of telepathy or of hyperaesthesia. . . . In all experimental work quantity, as well as quality, is important, and we hope, therefore, that not only will a considerable number of subjects present themselves, but that they will be willing to continue the experiments regularly, say, once a week, for at least two or three months, should it appear that interesting results are likely to be obtained.[70]

Something concerning the conditions of experimentation, and the nature of the results of recent experiments, is intimated in the same issue of the *Journal*:

> In most of the experiments in telepathy that are carried on at the Rooms, it is arranged for the agent to be in one room and the percipient in another.
>
> Though telepathic phenomena seem to occur fairly often, it is well known that they can only be experimentally demonstrated in rare cases, so that much of the time spent in such experiments is inevitably fruitless, producing merely negative results. Among a number of sets of experiments, however, tried during the last two years, a considerable proportion of successful results were obtained with two experimenters. (pp. 22-3).

This return of the Society, the publications of which contain the principal evidence for telepathy, to the problem, may be accepted as professional and official recognition of urgent need for further work in this field by psychical research. And, when it is recalled that Professor Richet insisted that his favorable results in experimental thought-trans-

[70] *Journal S. P. R.*, Feb. 1915, 17:32.

ference needed verification;[71] and that the old American Society for Psychical Research (1884-1889) which set itself this task discontinued its effort, after accumulating an imposing array of negative evidence, with an appeal by the Council for further coöperation, this trend in psychical research appears inevitable.

It is true that from the beginning certain leaders urged repeated experimental investigation of telepathy. Myers, in 1884, said:

> It is an obvious fact, but it is nevertheless a fact which we must repeat as often as possible, that in no way can psychical research be better aided than by constant and varied experiments on thought-transference in every form. (p. 217).[72]

Gurney's statement, made in the same year, has just been quoted in a foot-note.

In the discussion which followed F. W. H. Myers's report on the more recent English experiments on thought-transference, before the International Congress of Experimental Psychology at Paris, (August 10, 1890), Professor Richet said that he knew well of those experiments and had himself carried out a great number of a similar kind, reaching similar results; and (as reported by Dr. A. T. Myers) that

> Such experiments should be repeated widely and with the greatest care, for if the proof of thought-transference to which they led could be established, without doubt it would be one of the greatest discoveries of our time. (p. 182).[73]

And Professor Sidgwick "entirely agreed in the view [expressed by Professor Richet] that more experiments were urgently required."[73a]

Balfour Stewart, in his Presidential Address before the Society, in 1885, said:

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[71] Gurney, in his review of Richet's work (*Proceedings S.P.R.*, 2:242) pointed out that Richet was in error in interpreting his result as the degree of probability of the existence of a "Suggestion Mentale," instead of the "most probable measure" of the influence of the faculty *if* it exists; and that a repetition of Richet's experiments would yield "a valuable contribution." And on another page he said:

"He [Richet] insists that the experiments must be repeated; and the importance of this cannot be too strongly urged." (p. 257).

[72] Myers, F. W. H.: On a telepathic explanation of some so-called spiritualistic phenomena. *Proceedings S.P.R.*, 1884, 2:217-237.

[73] A. T. Myers: International Congress of Experimental Psychology. *Proceedings S.P.R.*, 1890, 6:171-182; *Cf.*, Congrès International de Psychologie Physiologique, Première Session, Paris, 1890, *Comte Rendu*, p. 153.

[73a] *Proceedings S.P.R.*, 1889, 6:182.

To my mind the evidence already adduced is such as to render highly probable the occasional presence amongst us of something which we call thought-transference or more generally telepathy; but it is surely our duty as a Society to continue to accumulate evidence until the existence of such a power cannot be controverted. (p. 66).[74]

That the large amount of evidence that was accumulated during the half-dozen years following these appeals has not proven conclusive has already been shown in the opinions quoted above. And during the more recent years equally urgent appeals for further investigation have been made.

Podmore, in 1894, wrote:

The first stage of our inquiry is not yet complete. It would be futile for us to debate what manner of new agency we propose to believe in until it is generally admitted by competent persons that the facts are not to be attributed to such recognized, if insufficiently familiar, causes as illusion, misrepresentation, and the subconscious quickening of normal faculties. More and varied experiments are wanted. (p. 394).[75]

James, in his presidential address to the English Society, in 1896, said:

We have published records of experiments on at least thirty subjects, roughly speaking, and many of these were strikingly successful. But their types are heterogeneous; in some cases the conditions were not faultless; in others the observations were not prolonged; and, generally speaking, we must all share in a regret that the evidence, since it has reached the point it *has* reached, should not grow more voluminous still.[76]

In the *Journal* for January 1900, under the title of "Premature Generalizations about Telepathy," the precautions necessary to be taken in experiments in telepathy are discussed, and the author continues:

It is true that the necessity for all these precautions was soon discovered by some of the earliest systematic workers of our Society (as may be seen by reference to the accounts of their experiments published in the early numbers of the *Proceedings*); but there is no doubt that the only way of advancing the subject further is to carry out many more experiments under the same stringent conditions as there described, or with any further precautions that experience might suggest. Accounts of such experiments would be most gladly received by the Editor. (p. 170.)[77]

[74] Stewart, Balfour: President's address. *Proceedings S. P. R.*, 1885, 3:64-68.
[75] Podmore: Apparitions and Thought-Transference. London, 1894.
[76] James: The address of the president. *Proceedings S. P. R.*, 1896-97, 12:4.
[77] *Journal S. P. R.*, Jan. 1900, 9:169-176.

Lodge, in 1909, wrote:

Why investigate that of which we are sure? Why conduct experiments in hypnotism or in telepathy? Why seek to confirm that of which we already have conviction? . . . The business of Science is not belief but investigation. Belief is both the prelude to and the outcome of knowledge. If a fact or a theory has had a *prima facie* case made out for it, subsequent investigation is necessary to examine and extend it. (p. 24).[78]

And in 1913:

If, however, direct first-hand laboratory experience of the rudimentary stages of such a faculty is wanted—as it ought to be—it must be looked and waited for, and experiments must be tried from time to time, as in any other branch of science. . . . I have now an apparatus set up for examining whether traces of the faculty exist widespread in normal people; and I shall make report to the Society for Psychical Research in due course.[79]

Bergson, in his presidential address of 1914, presupposed continued investigation when he said:

If telepathy is real, it is natural, and . . . whenever the day comes that we know its conditions, it will no more be necessary to wait for a veridical hallucination in order to obtain a telepathic effect than it is necessary for us now, if we wish to see an electric spark, to wait until the sky gives us a display during a thunderstorm. (p. 160).[80]

Thomas, in 1905, wrote:

In order to justify its existence as a body whose object it is to approach the study of these questions scientifically, the Society for Psychical Research must endeavor to supply these data and again take up the question of thought-transference. That other subjects attract a greater share of popular interest is clearly no reason for dropping the inquiry. Still less is absence of success, which appears to have prevented the publication of the trials between 1892 and 1901, a reason for discontinuing them. For it is clear that the smaller the measure of success under rigid conditions, the more probable is it that the conditions in earlier and more successful trials were lacking in some essential particular. (pp. 179-180).[81]

Tuckett, in 1911, while admitting "the *a priori* possibility of telepathy," maintains, in the face of results so far published, an attitude of skepticism:

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[78] Lodge: The Survival of Man, a study in unrecognized human faculty. New York, 1909.

[79] Lodge: Telepathy as a fact of experience. *Bedrock*, 1913, 2:57 ff.

[80] Bergson, Henri: Presidential address (before the S. P. R.), delivered May 28, 1913. *Proceedings S. P. R.*, 1914, 27:157-175.

[81] Thomas, Northcote W.: Thought Transference. London, 1905.

This attitude, however, does not prevent my hoping that further experiments in telepathy will be carried out by researchers trained in experimental psychology, for there is great need of such to throw adequate light on the question, which till then we must "leave in a decent obscurity." (pp. 307-8).[82]

And in 1912 he charges that the savants who have accepted the results of their investigations as proof of the fact of telepathy were satisfied with evidence that is not capable of verification (p. 182), and that they were inadequately equipped for such work:

They start on psychical research without the appropriate preliminary training which ought strictly to include knowledge of the methods of experimental psychology, of conjuring tricks, and of the vagaries of human nature such as is sometimes given by a medical career. (pp. 182 ff.).

The will to believe has made them ready to accept evidence obtained under conditions which they would recognize to be unsound if they had been trained in experimental psychology. (p. 204).[83]

Thus is the paramount issue in psychical research thrown by a keen and cautious critic into the laboratory of experimental psychology, and even a skeptical psychologist may be quoted in agreement with that assignment:

Scripture, in 1898, after discussing certain subtile psychological processes, said:

For thought-transference, therefore, all that is required is to find a subject who has an abnormally sharp ear, and, for your part, to think very intently on the word you wish transferred. It is not necessary that there shall be any intentional communication; if the investigators are sufficiently untrained in scientific psychological experimenting, and are inclined to attribute results to occult powers rather than to their own incapacity, the proofs of thought-transference inevitably follow. (pp. 259-260).[84]

And after laying down necessary precautions in conducting an experiment:

I have, I hope, said enough to make clear what an experiment is and what it is not. Such an explanation seems necessary at a time when so many really educated persons have put their faith in the results and deductions by the methods of psychical research. It is *a priori* impossible for an untrained man to make scientific experiments, and it is to be deeply regretted that persons of distinction

[82] Tuckett, Ivor Ll.: The Evidence for the Supernatural: a critical study made with "uncommon sense." London, 1911.

[83] Tuckett, Ivor Ll.: Psychical researchers and the will to believe. *Bedrock*, 1912, 1: 180-204.

[84] Scripture, E. W.: The New Psychology. London, 1898.

in other lines should undertake problems that require all the skill of a long-trained worker in the psychological laboratory.

The objectionable feature of psychical research does not lie in its subject of investigation. . . . The objections to psychical research lie in its unscientific methods of experimentation and in the air of occultism in which the whole is enveloped. If the investigators were trained in the psychological laboratory, we might expect interesting discoveries in regard to mind, while at the same time the repellent mysticism would disappear along with odic force, animal magnetism, thought-transference, and other ghosts. (pp. 68-9).[84]

This intimation of the reception the problem would meet in the usual psychological laboratory might be supplemented by a quotation from another psychologist:

Jastrow, who says:

I must not fail to point out, however, that experiments in thought-transference have one important, and that a logical, advantage over observations of coincidences; this is the possibility which they present of quite accurately allowing for the effect of chance. (p. 97).

. . . While I incline to the belief that the hypothesis of telepathy is, as usually advanced and in essence, an illegitimate one, I still regard it as possible that in the future some modification of this hypothesis may be found, which will bring it within the scope of a liberal conception of the scientific. (p. 101).[85]

As a matter of fact, however, the psychological laboratories in some universities[86] have proved hospitable to the investigation of telepathy by their conduct of systematic experiments on it, and the press[87] announces that in the Harvard laboratory, where arrangements have been made for psychical research, testing for "telepathic sensitiveness" in people in general is in progress. It may yet be possible to carry out the program recently suggested by The Hermit of Prague.[88] After noting that the integrity of the subjects in the older investigations has not always been found to be reliable, and that for the purpose "of convincing the world that the truth about telepathy has already been discovered, the recorded experiments of the Society for Psychical Research are almost without value" (p. 431), he recommends the appointment of a commission to induce experimental psychologists, who are best equipped to discover telepathy, to take up the investigation in laboratories all over the world. Whenever one finds success, let him pass on the "sensitive" from one laboratory to another, until it is agreed that fraud and error are eliminated and telepathy is established as either a fact or a delusion.

[85] Jastrow, Jos.: Fact and Fable in Psychology. Boston, 1900.

[86] Notably in Clark and Cornell.

[87] *The Unpopular Review,* Jan.-Mch. 1917, 7: 210.

[88] The Hermit of Prague: Materialism and telepathy. *Bedrock,* 1914, 2: 423-434.

EXPERIMENTS ON THOUGHT-TRANSFERENCE

Since 1882, when organized investigation of telepathy began, prominent investigators and students of psychical phenomena have suggested that this super-normal process is a common faculty shared by all men.

F. W. H. Myers, 1884:

If we find telepathy in mesmeric and spontaneous trance, we may infer that it is not inseparably linked with the ordinary stream of normal consciousness. If it appears as an element of consciousness or quasi-consciousness of abnormal states, which themselves form the mere *lacunae* in the main life-memory, it may be surmised to exist beneath the threshold of consciousness in normal states also. (p. 220).[89]

Charles Richet, 1884:

La suggestion mentale est l'influence que la pensée d'un individu exerce dans un sens déterminé, sans phénomène extérieur appréciable à nos sens, sur la pensée d'un individu voisin. (p. 615).

De ces chiffres, de ces expériences peuvent, je crois, se déduire, en toute rigueur, cette conclusion: Chez des personnes adultes, en bonne santé, non hypnotisées, ni hypnotisables, il est possible que la suggestion mentale se fasse sentir. Cette suggestion mentale est même, dans une certaine mesure, probable; mais avec un degré de probabilité qui ne dépasse guère 1/16 (1/10?). (p. 632).[90]

Edmund Gurney, in 1886:

If it [telepathy] exists, we have no reason to expect it to be extremely uncommon; on the contrary, we should rather expect to find an appreciable degree of it tolerably widely diffused.[90a]

Bergson, 1913:

If telepathy is a real fact, it is a fact that is capable of being repeated indefinitely. I go further: if telepathy is a real fact, it is very possible that it is operating at every moment and everywhere, but with too little intensity to be noticed, or else it is operating in the presence of obstacles which neutralize the effect at the same moment that it manifests itself. (p. 160).[91]

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[89] Myers, F. W. H.: On a telepathic explanation of some so-called spiritualistic phenomena. *Proceedings S. P. R.*, 1884, 2:217-237.

[90] Richet, Charles: La suggestion mentale et le calcul des probabilités. *Revue Philosophique.* Paris, 1884, 18:609-674.

[90a] Phantasms of the Living, vol. I, pp. 84-5.

[91] Bergson, Henri: Presidential address (before the S. P. R.), delivered May 28, 1913. *Proceedings S. P. R.*, 1914, 27:157-175.

Consequently, they have frequently urged, as the reader has perhaps noted in the quotations some pages above, continued experimentation with normal subjects. For example, Sir Oliver Lodge, in 1909, wrote:

> Another thing on which I should value experiments is the detection of slight traces of telepathic power in quite normal persons—in the average man for instance, or, rather more likely perhaps, in the average child. The power of receiving telepathic impressions *may* be a rare faculty existing only in a few individuals, and in them fully developed; but it is equally possible, and, if one may say so, more likely, that what we see in them is but an intensification of a power which exists in every one as a germ or nucleus. If such should be the fact, it behooves us to know it; and its recognition would do more to spread a general belief in the fact of telepathy—a belief by no means as yet universally or even widely spread—than almost anything else. (pp. 32-3).[92]

The investigations included in Part I were designed primarily to put this hypothesis to further test. Other, subsidiary, aims were met through the detail of method employed, and will be found stated in the introductory paragraphs to each division. The following table shows the number of reagents (percipients) employed, the nature of the reagent, the material guessed at, and the number of experiments:

Division	Number of Reagents	Nature of Reagents	Material guessed at	Number of Experiments
I	1	Normal	Lotto-Block Numbers	1000
II	100	"	Playing Cards	10000
	5	"	" (Corneal reflection)	500
	15	"Psychic"	"	1000
III	24	Normal	"Staring"	2400
Totals	145			14900

[92] Lodge, Oliver: The Survival of Man, a study in unrecognized human faculty. New York, 1909.

I. GUESSING OF LOTTO-BLOCK NUMBERS.[93]

Introduction.

Quotations from authoritative sources have already been offered the reader for the purpose of acquainting him with the present unsettled status of the telepathic hypothesis, but no effort has been made to place before him the evidence upon which to exercise his own judgment.

It will be recalled that experimental evidence in favor of the hypothesis is regarded as imposing but not readily acceptable. Two reasons for caution may be mentioned: (1) The hypothesis not being consistent with a psychological law which we may call "the principle of the sensorial gateway," must be supported by evidence sufficiently overwhelming to controvert that law in order to become acceptable; (2) Trustworthy negative results already published are equally imposing, with respect, at least, to the question of the general distribution of the telepathic function.

The reader may get a fairly accurate idea of the present situation from the following series of researches and their resultant criticisms:

(1) Extensive experimentation[94] conducted by Mrs. Henry Sidgwick upon Lotto-Block guessing by hypnotized subjects, yielded results favorable for thought-transference. If the results of good and bad days are combined and the two digits of each number counted as separate numbers, (as in Table VII, p. 168), the Right cases are 30% of the total 1356 guesses, as against a probability of about 11%; but if the results of good days are considered separately, for two reagents (P. and T.) we get[95] 27% Right cases on the two-place numbers out of 374 experiments, as against the probability of 1.23%. The impressions seemed to come in visual form, yet the sense of sight could not have been operative. In a discussion of the possible contribution of the senses, all seemed to be ruled out; the least improbable, in case any could have been active, was said to be that of hearing, made effective through the

[93] Conducted during the year 1912-13.

[94] Sidgwick, Professor and Mrs., and Smith, G. A.: Experiments in thought-transference. *Proceedings S. P. R.*, 1889-90, 6: 128-170.

Sidgwick, Mrs. Henry, and Johnson, Miss Alice: Experiments in thought-transference. *Proceedings S. P. R.*, 1892, 8: 536-596.

[95] From Tables I and II, *op. cit.*, pp. 146, 150.

unconscious whispering on the part of the agent; but tables failed to show this sense operative, since there was no heaping up of errors according to the similarity of sounds of the digits.

(2) Hansen and Lehmann,[96] taking the cue of auditory communication, conducted a similar investigation on Lotto-Block guessing, changing the conditions of experimentation to test its efficacy; large parabolic reflectors which augmented the intensity of the sound fourteen times, were set up with axes coinciding and foci a meter apart; the agent sat facing his reflector, his mouth at its focus, and permitted himself to represent the number to himself in inner speech in a way which he calls involuntary whispering (unwillkürliches Flüstern); to a bystander neither sound nor movement of the closed mouth and lips was observable. The percipient, in the normal state and sitting with his ear at the focus of his reflector, was influenced in his guessing so that in 86 and 80 experiments in which the chances were 1:1.2, 34% and 32% respectively of the guesses were correct; and, when the digits were combined, the probability being about 11%, there were 54% Right cases in the 1000 guesses. They compared the errors in this series with those in the series of guesses upon two-place numbers presented visually by a tachistoscope so quickly as to remain indefinite, together with those of the Sidgwick experimentation, and concluded that the results of the latter were undoubtedly owing to involuntary whispering. The hyperaesthesia of the hypnotized reagents and their favorable positions for sound perception were conditions said to be accountable for success even when the agent and percipient sat in different rooms.

(3) Critique of the latter by Professor Sidgwick[97] and by Professor James.[98] The former pointed out that (*a*) by trial with a reagent practiced in perceiving faint whispers, and by watching the former agent for indications of movement of the vocal organs in the submaxillary regions, no involuntary whispering could be detected. (*b*) Owing to faulty methods of comparison of errors, Hansen and Lehmann's conclusions are "quite inconclusive," since the agreements of the most fre-

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[96] Hansen, F. C. C., und Lehmann, Alfred: Ueber unwillkürliches Flüstern. *Philosophische Studien,* 1895, 11:471-530.

[97] Sidgwick, Professor Henry: Involuntary whispering considered in relation to experiments in thought-transference. *Proceedings S.P.R.,* 1896-97, 12:298-315.

[98] James, Wm.: Review of Hansen and Lehmann's work, in *Psychological Review,* 1896, 3:98-100; see also, review of Sidgwick's critique, *Psychological Review,* 4:654-5, and Lehmann and Hansen on the telepathic problem. *Science,* 1898, 8:956.

quent substitutions between the results of the Sidgwick experimentation and the results of a series in which the agent and percipient were located in different houses, in which the correct guesses could be regarded as governed by pure chance, and in which the whispering could not be effective, are as close as between the former and the results of the Hansen and Lehmann experimentation. (*c*) Involuntary whispering could not have been instrumental to the success of the experiments in which the agent and percipient were separated by a closed door and a considerable space (10 to 20 feet).

James agrees with Sidgwick that Hansen and Lehmann did not prove their point; he reports that he made up 1000 guesses on two-place numbers, under the condition of whispering with lips closed, and found that substitutions do not agree more closely with the Sidgwick results than with chance. He notes that Parish [99] also agrees with Sidgwick.

(4) Lehmann wrote in reply to James's inquiry asking him what he himself thought of his conclusions in the light of the criticisms in question:

"Your own as well as Professor Sidgwick's experiments and computations prove beyond a doubt that play of chance had thrown into my hands a result distinctly too favorable to my theory; and that the said theory is consequently not yet established (bewiesen)." [100]

(5) Lehmann, in a recently revised work,[101] still holds to his "Unwillkürliches Flüstern" hypothesis as being sufficiently proved to hold for the Sidgwick results; but grants Professor Sidgwick, for the success of the experiments in which agent and percipient were separated by a door and a considerable distance, another factor: "Andere Umstände für eine solche Telepathie sprechen, deren Natur noch gänzlich unbekannt ist"; namely, subliminal impressions, etc., which he discusses in Kapitel 34 under the title of "Das Eingreifen des Unbewussten in das Bewusstsein." (p. 512).

The results favorable for thought-transference still stand unexplained. It is true that there have been from the earliest researches

[99] Parish, Edmund: Hallucinations and Illusions. New York: Scribners, 1897, p. 320, note.

[100] James: Messrs. Lehmann and Hansen on telepathy. *Science*, 1899, 9:654-5.

[101] Lehmann, Alfred: Auberglaube und Zauberei. 2te Auf., Stuttgart: Enke, 1908, p. 464.

critics [102] who have not wearied in pointing out errors of experimentation and observation to which the favorable results are probably due. But in spite of illuminating knowledge of unconscious muscular activity, subliminal impressions, suggestion, mental habits, variability of experimental chance, etc., there has been no thorough-going research by psychologists, besides the abortive one by Hansen and Lehmann, to determine the conditions and processes responsible for results favorable for thought-transference without contact.

It would be gratifying to know whether the percipient is successful only when in an abnormal condition as in hypnosis or trance, or has some advantage for subliminal impression from the reflection of the cornea or vibrations of the tympanic membrane (if such occur with strong auditory imagery or with osseous disturbance accompanying kinaesthetic imagery—one of which is suggested by the finding of Abbott's [103] investigation of the phenomena of a certain trumpet medium).

Richet at one time was quite confident that the equivalent of thought transference, "suggestion mentale," was to a small degree (3% to 10%) a common human capacity. But Preyer [104] showed that the deviations from probability upon which Richet had based his conclusions are not only equalled but surpassed by experimental chance (in lottery drawings in series of equal length to that of Richet). And the 11,130 guesses upon digits, by 27 different percipients, collected by the Committee on Thought-Transference, of the American S. P. R., indicated by their 10.17% of Right cases, as against the 10% of theoretical probability, that Richet was probably in error. James,[105] in his presidential address before the S. P. R., said of Richet's supposition: "I am inclined to think [it is] not very well substantiated. Thought-transference may involve a critical point, as physicists call it, which is passed only when certain psychic conditions are realized, and otherwise not reached at all—just as a big conflagration will break out at a certain temperature, below which no conflagration whatever, big or little, can occur." (p. 4). And Lehmann, in his chapter "Das Gedankenlesen und die Gedankenübertragung," says: "Aus später zu erörtenden Gründen ist aller Wahr-

[102] Among others, Preyer, Baird, Braid, Carpenter, Morselli, Vaschide, Marbe, Jastrow, Hall, Tanner.

[103] Abbott, David P.: The history of a strange case. *Open Court,* June 1908.

[104] Preyer, W.: Die Erklärung des Gedankenlesens. Leipzig: Grieben, 1886, p. 70.

[105] James: Address by the president. *Proceedings S. P. R.,* 1896-97, 12:2-10.

scheinlichkeit nach eine Mitwirkung der telepathischen Kräfte indes nur dann anzunehmen, wenn der Empfänger hypnotisiert ist oder sich in einem ähnlichen Zustande befindet." (p. 464).[106]

The object of this division of our experimentation is to get a "norm" (or standard measure) for normal persons, with which we may compare results from "sensitives." The subjective conditions of guessing are scrutinized, and by carrying on "control" experiments we hope to establish an inductive probability with which we may compare our results as well as with the theoretical probability.

Method.

Our blocks are of light hard wood, 15 mm. in diameter, 8 mm. high, and the black Arabic numerals are printed in heavily shaded Roman type, 8 mm. high. As in the former researches, the two-place numbers up to and including 90 were used.

The reagent (percipient) sat in an arm-leaf chair with his back to the experimenter (agent), and after each experiment (each guess of a double number) noted introspections in a tabulated form under the following headings: (*a*) Was the mind in a thoroughly receptive mood? (*b*) Grade of certainty of judgment, or vividness of imagery, (*c*) Kind of impression (visual, auditory, or kinaesthetic), (*d*) Temporal course of the impression (sudden or slow in appearance, early in the period or late, persistent, recurring), (*e*) Spatial attributes (where apparently located). The grading was to be *A, B, C, D,* in descending order; and in case certainty of judgment differed from vividness of imagery, two grades were to be given.

The period during which the reagent was to seek an impression of some number from 10 to 90, was at first 20 seconds, but upon the request of the reagent was later reduced to 15 seconds. The reagent during this period shaded his closed eyes with his hand, rested his head comfortably with elbow upon the arm of the chair, and sought a quiet, receptive state of mind. He knew that the block might not be looked at by the experimenter until after his guess was recorded by himself, and that if its number was held in imagery the latter might be visual, kinaesthetic, or auditory, or combinations of these.

The experimenter sat facing the reagent's back, drew from a bag a Lotto-Block, and, if the numbered side came up, made ready to hold it

[106] Lehmann: Aberglaube und Zauberei, pp. 458-465.

in some kind of vivid imagery, shook the dice-box, tapped with his pencil once to inform the reagent that the period of impression began, then held or did not hold imagery of the number; and after 15 seconds tapped twice to close the period of impression. When the numbered side of the block came up, imagery was held according to the face cast by the die, as follows: (1) Visual impression; (2) Kinaesthetic image (care being taken to avoid an auditory accompaniment, and also any movements of pronunciation great enough to be felt; (3) Auditory imagery (stripped of its usual kinaesthetic accompaniment); (4) Combination of 1 and 2; (5) of 1 and 3; (6) of 2 and 3. In order that there would be no confusion, the experimenter kept before him a card with the kinds of impressions, or imagery, tabulated by number, upon it. In case the blank side of the block was drawn, the dice-box was shaken, and the experiment progressed in every way like its alternate; except that the experimenter refrained from thinking of numbers (by musing upon an ocean scene). After the reagent had recorded his guess and while he was writing his introspections, the experimenter recorded the block-number, and the face of the die, and indicated whether the number was imaged. Accompanying the imaging of the number was a determined set of the will that it be communicated to the reagent. The distance between experimenter and reagent was changed every 20 experiments, irregularly over the following distances in meters: 1, 2, 3, 4.6, 6, 10.

The reagent was given all the time he wished to note his introspections, which at the beginning was up to about 10 minutes; after he became more familiar with the procedure the time settled down to half a minute, and the rate of the experiments became one a minute. A sitting was never continued after the fatigue point had been reached; the number of experiments ranged from 20 to 50, but was rarely over 30 during the first half of the experimentation. The sittings took place in the first hour of the afternoon, on alternate days, three times a week. The experiments numbered 1000. The conditions of quiet and regularity of procedure usual in the psychological laboratory obtained. Neither experimenter nor reagent knew how the results were coming out until the whole ten series had been concluded.

The reagent, Harold A. Hughes, registering from Massachusetts, was a major in the Department of Psychology, and was doing advanced laboratory work. He is versatile and responsive by nature. His replies to a questionnaire record that (*a*) He sometimes has the feeling of be-

ing stared at, with the conviction that the feeling can be relied upon; (*b*) He has not had premonitions of important events, but knows persons who have had; (*c*) He has mental pictures of coming or distant events, and (*d*) he sees pictures in water-glasses, etc.; (*e*) He has found that he can cause a person in front of him in an audience to turn around by "willing" it; (*f*) He has never been hypnotized. (*a*) and (*e*) testify to his belief in his own power in a phase of telepathic experience, and (*b*) supports (*a*) and (*e*) in indicating his belief in telepathic phenomena; (*c*) and (*d*) indicate probably nothing more than strong visual imagery. This student seemed, therefore, a good reagent for our purpose. Nothing was wanting in the seriousness and faithfulness with which he carried forth his end of the research.

The favorable state of mind of the reagent during the interval given for impression can be seen from his written description: "When the signal came I made my mind a blank and just waited for a number to be impressed upon me. . . . I did not consider different alternatives, nor did I try to 'guess' some number. I just let some number enter my mind—without trying in the least to make it any certain number."

The experimenter (the writer) took his advanced work in science in psychology, from which department he has received the higher degrees, and has had considerable experience in the laboratory, both as reagent and as experimenter. While at a small college he made some experiments, during February 1891, in hypnotism (then known to him as Mesmerism), and the records of his diary support his memory to the effect that experiments in telepathy and clairvoyance were successful. He had ten subjects, some of whom were hypnotized a half-dozen times. But that was before he had received any training in science, and he realizes that he was competent neither as an experimenter nor as an observer, and he is therefore somewhat agnostic as to the results of his experimentation; yet, when he re-reads his notes, he cannot avoid the conviction that here is a worthy field for scientific investigation. His former success has inspired him with confidence in his power to maintain the psychical conditions of an agent, favorable for thought-transference; it was in this spirit that he performed his part of the present experimentation.

Results.

The numerical results were tabulated under the headings of the "Number Not Imaged" and "Number Imaged" experiments. It is to be noted therefore that the averages of the "Number Imaged" experiments may be compared with those of an Experimental Probability as well as with the Theoretical Probability in order to determine whether any cause besides chance has been operative toward Right cases.

The complete and partial successes for each of the 10 Series, of 100 experiments each, are tabulated in Tables I and II:

TABLE I. NUMBER NOT IMAGED.

Series of 100	Whole No. Correct	Ten's Correct	Unit's Correct	Ten's for Unit's	Unit's for Ten's	Transposed	Total N. I.
1.	1	8	9	0	2	0	49
2.	1	4	1	2	3	1	51
3.	1	4	5	4	4	1	57
4.	0	5	4	3	3	1	47
5.	0	8	2	7	4	0	48
6.	0	3	5	3	3	1	54
7.	0	2	2	4	4	0	41
8.	0	5	2	4	7	0	47
9.	2	6	6	4	4	0	51
10.	2	6	6	4	4	0	51
Totals	6	50	40	37	40	4	502

TABLE II. NUMBER IMAGED.

Series of 100	Whole No. Correct	Ten's Correct	Unit's Correct	Ten's for Unit's	Unit's for Ten's	Transposed	Total I.
1.	2	4	4	0	1	0	51
2.	0	5	8	2	5	0	49
3.	1	8	1	3	2	1	43
4.	1	7	4	4	4	0	53
5.	0	8	4	2	1	1	52
6.	1	5	3	6	3	1	46
7.	0	7	5	6	2	1	59
8.	0	3	7	6	3	1	53
9.	0	5	6	3	3	1	43
10.	0	7	4	1	6	0	49
Totals	5	59	46	33	30	6	498

TABLE III. COMPARISON.

	Whole No. Correct	Ten's Correct	Unit's Correct	Ten's for Unit's	Unit's for Ten's	Trans-posed	Total
Number Imaged.....	5	64	51	33	30	6	498
	1.00%	12.8%	10.2%	6.6%	6.0%	1.23%	
Number Not Imaged.	6	56	46	37	40	4	502
	1.18%	11.1%	9.1%	7.4%	8.0%	0.8 %	
Probability.....	1.23%	12.5%	10.0%	6.9%	6.9%	0.85%	

The totals of the second and third columns of Table III include, of course, the totals of the preceding column, since when a whole number is correct both the ten's and the unit's digits are also correct.

In the calculation of Probability, the ratios have been changed to per cents. A whole number stands one chance in 81 of being correct; a ten's digit 1:8.1, since there are but eight, each with its ten units, and the number 90; a unit's digit 1:10; a ten's digit for a unit's digit, 1:14.5, since among the 81 numbers there are 25 which cannot be transposed, as, 10, 20, 30 . . ., 19, 29, 39 . . ., 11, 22, 33 . . ., which reduces the chance of success to 56/81 of 1:10; a unit's digit for a ten's digit, also 1:14.5; for the same reason, a transposed number, 56/81 of 1:81, or 1.117.

Tables IV and V show the distribution of the guessed upon the drawn numbers, and are comparable with the tables published by Mrs. Sidgwick and Hansen and Lehmann, except that these authors combined the guesses on the unit's and ten's digits. We have not done so for the reason that the reagent differed from those of Mrs. Sidgwick in that he got his impressions as double instead of single numbers. For the purpose of showing number habits separation is desirable, and, as is indicated above, the probability ratios of the two are different.

TABLE IV. UNIT'S DIGIT. NUMBER NOT IMAGED.

	Guessed										
Drawn	*0*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	Total
0	5	4	8	10	5	3	3	4	3	3	48
1	5	3	6	2	5	4	9	6	5	6	51
2	4	4	4	3	3	5	4	8	6	6	47
3	6	7	5	6	5	3	5	6	3	7	53
4	8	3	7	4	6	7	3	6	5	9	58
5	5	2	8	4	3	6	7	7	6	2	50
6	5	6	5	4	4	5	3	3	3	3	42
7	9	6	8	7	3	5	9	5	2	9	62
8	2	3	10	5	1	4	5	5	4	3	42
9	7	7	3	2	7	4	4	7	4	4	49
Totals	56	45	64	47	42	46	52	57	41	52	502

TABLE V. TEN'S DIGIT. NUMBER NOT IMAGED.

Drawn	Guessed 1	2	3	4	5	6	7	8	9	Total
1	**13**	18	17	6	3	3	4	3	0	67
2	15	**9**	17	14	2	8	5	4	2	76
3	13	11	**14**	11	3	5	7	6	0	70
4	2	8	14	**10**	8	12	8	2	2	66
5	10	10	11	13	**3**	2	4	7	0	60
6	9	6	9	7	4	**2**	5	2	2	46
7	15	14	11	10	9	3	**4**	1	1	68
8	8	12	6	7	4	5	3	**1**	0	46
9	1	0	1	0	0	0	1	0	0	3
Totals	86	88	100	78	36	40	41	26	7	502

TABLE VI. UNIT'S DIGIT. NUMBER IMAGED.

Drawn	Guessed 0	1	2	3	4	5	6	7	8	9	Total
0	**6**	8	9	7	7	5	3	7	5	8	65
1	4	**4**	5	7	9	7	8	3	3	13	63
2	6	2	**4**	6	5	2	5	5	2	1	38
3	7	6	4	**5**	4	5	3	3	9	2	48
4	8	6	5	2	**10**	1	5	5	4	1	47
5	4	3	3	4	1	**6**	4	10	6	4	45
6	3	3	11	8	2	5	**4**	4	4	3	47
7	7	4	12	3	4	4	4	**8**	6	5	57
8	5	8	6	2	4	4	3	2	**0**	3	37
9	8	6	7	3	5	1	5	6	6	**4**	51
Totals	58	50	66	47	51	40	44	53	45	44	498

TABLE VII. TEN'S DIGIT. NUMBER IMAGED.

Drawn	Guessed 1	2	3	4	5	6	7	8	9	Total
1	**4**	10	13	9	4	5	6	1	1	53
2	9	**18**	12	10	2	6	5	8	1	71
3	9	10	**12**	12	4	4	5	7	0	63
4	6	15	8	**11**	5	3	6	5	2	61
5	10	12	9	5	**6**	6	5	3	1	57
6	12	11	10	7	6	**3**	4	3	1	57
7	9	8	11	13	7	5	**8**	4	2	67
8	8	9	13	10	10	6	4	**2**	1	63
9	0	1	0	1	0	2	2	0	0	6
Totals	67	94	88	78	44	40	45	33	9	498

An examination of Tables I-III does not warrant the conclusion that the judgments of the reagent were influenced by the imagery in the mind of the experimenter; nor do the errors in the Tables IV-VII show it, by similarity of sight or sound of the digits. But, supposing that some unknown cause counteracted a telepathic influence, and thus kept it from being shown in these particular tables, other tests are at hand. If there was a transference of thought at all, it is reasonable to suppose that successful guesses would be positively correlated with the high grades of certainty in the "Number Imaged" experiments, but not in the "Number Not Imaged" experiments; that some particular form of imagery would, for this reagent, be more efficient than the others; that some particular distance would be the more favorable; and that in successful guesses the imagery in the mind of the reagent would conform in kind with that in the mind of the experimenter.

If we call Right cases the experiments in which the whole number, the ten's digit, or the unit's digit, is right, the two following tables show the relationship between Right cases and Certainty of guesses.

Table VIII shows the distribution of the three grades over the "Not Imaged" and the "Imaged" experiments (*B* is the highest, *D* the lowest grade of certainty):

TABLE VIII.

	B	%	C	%	D	%
Total Grades.....	352		557		91	
R Cases, Number Not Imaged...	31	8.8%	50	9.0%	14	15.4%
R Cases, Number Imaged.....	39	11.1%	62	11.1%	9	9.9%

Almost as many highly graded guesses (lacking but 2%) were made in the "Number Not Imaged" experiments.

Table IX shows the distribution of the highest and lowest grades over the three kinds of successes, for the "Number Not Imaged" and the "Number Imaged" experiments:

TABLE IX.

	Grade	Whole No.	Ten's	Unit's	Total	Grand Total	%
Number Not Imaged.....	B	2	17	12	31	180	17.2
" " "	D	0	9	6	15	50	30.0
Number Imaged.....	B	1	26	12	39	172	22.6
" "	D	1	4	4	9	42	21.4

The greater number of *B*'s on the ten's digit successes in the "Number Imaged" experiments is the only advantage the table shows; this, however, is but moderate, and is probably offset by the facts (1) that about the same proportion of *D*'s as of *B*'s have correlated with successes, (2) that no advantage is shown on the unit's digit, and (3) that the grades of the few complete successes are somewhat lower in the "Number Imaged" than in the "Number Not Imaged" experiments.

The data seem to warrant the conclusion that Certainty of guesses does not correlate with Right cases.

Whether any sort of imagery was more efficient than others, may be determined by a distribution of the Right cases over the die-spots, since the latter determined the respective forms of imagery:

TABLE X.

	Die	Whole No.	Ten's	Unit's	Total	Grand Total	%
Number Not Imaged.....	1	1	7	7	15	78	19.2
" " "	2		13	7	20	75	26.7
" " "	3		7	4	11	91	12.1
" " "	4	1	9	6	16	87	18.4
" " "	5	1	7	6	14	86	16.3
" " "	6	3	7	10	20	85	23.5
Number Imaged.....	1	1	9	5	15	81	18.5
" "	2		9	6	15	95	15.8
" "	3		8	10	18	84	21.4
" "	4		16	8	24	85	28.2
" "	5	3	11	11	25	82	30.5
" "	6	1	6	6	13	71	18.3

Although the per cents show some variation, it appears to be governed by chance: the 4-spot and the 5-spot seem to be more efficient, but the same might be said of the 2-spot in the "Number Not Imaged" experiments. Since the latter is obviously a chance variation, the former, being but slightly larger, must be held to be probably chance variations also. The imagery determined by the 4-spot and the 5-spot was, respectively (Visual impression) + (Kinaesthetic image) and (Visual impression) + (Auditory image); since the visual impression was common to both, one would expect its efficiency alone to be greater than that of the Kinaesthetic or Auditory imagery alone, and 1 is but intermediate between 2 and 3, and is lower than 1, 2, and 6, in the upper part of the table; moreover, the experimenter found 5 the most difficult imagery to hold vividly, as any one else probably will if he tries it. The

imagery Hansen and Lehmann tried to prove accountable for the successes in Mrs. Sidgwick's research is 2, which in the table is correlated with the lowest per cent. The experimenter, being of the kinaesthetic type, and finding that imagery more easily held in a vivid form, might reasonably have expected the highest per cent on 2.

We are probably safe in concluding that no particular form of imagery was more efficient than any other in conditioning Right cases.

Whether some particular distance was most favorable may be determined from the following table, which distributes the Right cases over the six distances used:

TABLE XI.

Meters	1	2	3	4.6	6	10
Number Not Imaged—total cases.....	89	34	94	71	104	110
" " " —total right cases..	22	4	23	16	16	16
Per cent.....	24.7	11.8	24.5	22.5	15.4	14.6
Number Imaged—total cases.....	91	26	86	109	96	90
" " —total right cases.....	18	7	16	26	23	18
Per cent.....	19.8	26.9	18.6	23.9	24.0	20.0

But few experiments were made on the 2-meter mark, and these per cents may be disregarded. If any distance is more favorable than another it is 6 meters; but as much advantage is shown by 1 meter in the "Number Not Imaged" experiments. The latter being a chance variation, the former probably is also.

We may probably safely conclude that no particular distance was the more favorable for Right cases.

Number Habits.

As has been pointed out by Mrs. Sidgwick,[107] number habits on the part of the reagent alone cannot, except by chance, augment the number of Right cases; they would even resist a telepathic influence and tend to keep it from operating in the process of guessing and from being shown in the numerical results. They are of interest, however, in revealing some of the subjective conditions of guessing, on the part of individual reagents.

An inspection of Tables IV-VII reveals the fact that guesses were not distributed equally on the double numbers. The totals of the re-

[107] *Proceedings S. P. R.*, 6:170.

agent's guesses below 50 are much greater than above 50. (See Tables V and VII). The ratios are:

Not Imaged.....327 : 162 = 2.02
Imaged .....352 : 143 = 2.46

The 30's in the "Number Not Imaged" and the 20's in the "Number Imaged" experiments were guessed the more often; while the average number of guesses per number is 6, of the forty numbers below 50

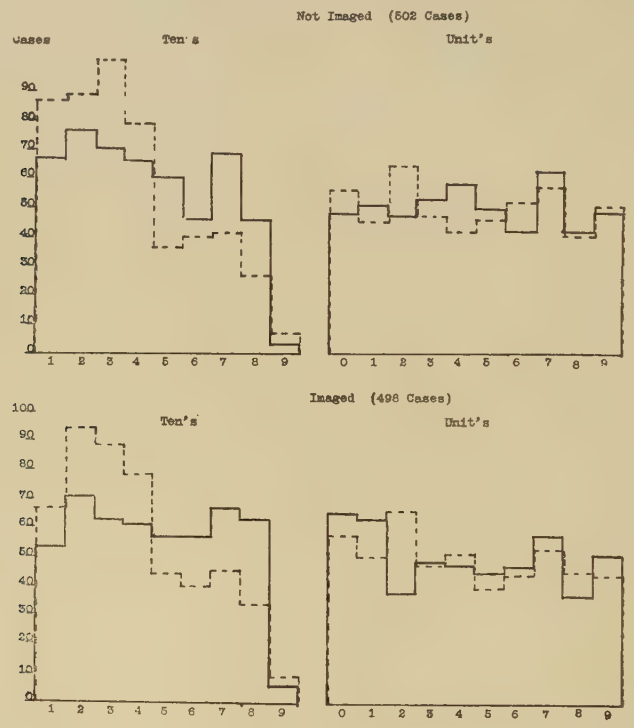

Plate II.— Showing Mental Habit in the Guessing of Two-place Numbers. Guesses are represented by broken lines; drawings by solid lines.

twenty-seven in the "Number Not Imaged" experiments, and twenty-one in the "Number Imaged" experiments, were guessed over 7 times; the number 37 in the former was guessed 16 times, the number 32 in the latter 15 times; in the former the numbers 68 and 86, and in the latter the number 10, were not guessed at all.

Of the unit's digits, 2 and 7, in the order named, show a slight preference. Plate II shows the difference between the guessing and the drawing of the digits.

Imagery.

As was noted above, the reagent knew in what kinds of imagery the experimenter would hold the numbers. Whether it was on this account or because he customarily has vivid imagery in different modes, he differed from our more naïve reagents in recording three, and later four, kinds of imagery as the forms in which the number came into his mind. In the 4th 100 he distinguished Kinaesthetic-Auditory from Auditory, and fell off in Visual imagery from 20% to 6%; the Kinaesthetic-Auditory increasing from 0% to 21%. The following table shows the change that occurred in his imagery, as well as its distribution over the various kinds.

TABLE XII.

1. Visual	4. 1 + 2	Kinds of Imagery..	1	2	3	4	5	6
2. Kinaesthetic	5. 1 + 3	Avg. of first 300...	22%	37%	40%	0.	0.	0.3%
3. Auditory	6. 2 + 3	Avg. 4th-10th 100's..	6%	40%	32%	0.4%	0.1%	22%

Within these two periods the ratios are very uniform, which would hardly be the case unless there had been a change in the reagent's impressions and the latter had been pretty faithfully described. The only change due to keener analysis of his experience is the sorting out of the Kin.-A. images from the Auditory ones. The Kin.-A. imagery involves imagery of movements of pronunciation and of the sound of his own voice; the Auditory alone involved the imagery of a voice (not his own) whispering the number to him or speaking it from a position in front and to the right or from a point in the top of his head. The Visual imagery pictured the figures written on a blackboard about a meter and a half in front, in white 2 inches high, or printed on small blocks the size of a dime (a ten-cent-piece), held in the experimenter's hand. Some variation from the rule was the appearance of vivid memory images; as 20 on a $20 bill, 22 on a red background (the front of the campus electric car), or the 33 of the Hudson automobile; these, however, occurred very rarely.

With respect to the time and manner of appearance of the imagery, tabulation shows that the Visual imagery usually appeared suddenly, and in the early part of the period given for the impression; Kin. imagery changed by the 4th 100 from about 50% of the cases coming late to 75% coming early, and it came suddenly; the Auditory changed in the 2d 100 from about 2/3 early to about 2/3 late, and it was sometimes slow in forming; the Kin.-A. imagery came regularly suddenly and early.

Tabulation of the imagery of the successful guesses shows that the kind of imagery in the experimenter's mind did not coincide with the kind of imagery in the reagent's mind more often in the "Number Imaged" experiments than did the corresponding dice-numbers in the "Number Not Imaged" experiments. And special cases of vividness calling for extra note either fall upon experiments when the block was not looked at or do not agree in imagery with that held in the experimenter's mind.

The feeling of certainty that the impression had an objective source seemed to depend upon

1. Vividness of the imagery,
2. Early appearance,
3. Flashing out,
4. Persistence or recurrence.

This is true even of the memory images of 20, 22, etc. (It may be mentioned in passing that these special numbers were not guessed more often than their neighbors.)

Conclusion.

Although the conditions of our experimentation, and the attitude and training of the reagent, seemed favorable for thought-transference, the results of a thousand experiments indicate that the number of successful guesses is not beyond either experimental or theoretical probability; that the feeling of certainty with which judgments are made does not correlate with Right cases; that no particular form of the experimenter's imagery was more efficient than others; that no particular distance was more favorable than another; that in the successful cases the kind of imagery in the mind of the reagent did not conform to the kind of imagery in the mind of the experimenter more often than to the corresponding die-spots in the experimental probability experiments; that the reagent was successful in keeping his mind in such a receptive condition that impressions seemed to have an objective source; and that this feeling of certainty of the chance of a guess being right seems to depend upon the vividness and behavior of the imagery constituting the impression.

The results of these series of experiments, then, in which a reagent, in the normal state and under conditions supposedly favorable for thought-transference, made 498 guesses upon Lotto-Block numbers (from 10-90) when the number was vividly imaged by the experimenter, and

502 guesses when the number was unknown to the experimenter, without causing in the results a significant deviation in either group of experiments from theoretical probability, support Professor James in his judgment that Professor Richet's hypothesis to the effect that thought-transference is a common capacity, to be found in any long series of guessing in the presence of some one who knows what is being guessed at, is probably wrong.

The qualitative results of these experiments, afforded by analyses of introspections, are of psychological interest in establishing the fact that normal persons have experiences which they refer with varying degrees of certainty to an objective source but which in reality do not depend upon it. This tendency to project subjective experience is obviously related to the psychical processes of illusion and hallucination, as an incipient function to an active one, and is to be regarded as both common and normal.

II. GUESSING OF PLAYING-CARDS.

SERIES I. REAGENTS NORMAL.

Successful results in card-guessing by normal reagents (subjects, percipients) have been reported in not a few experiments on Thought-Transference; notably, (1) by the Committee on Thought-Transference,[108] of the Society for Psychical Research, in England, in 1882, who reported 22 out of 248 trials correct, or 8.87%, as against the probability of 1.92%; (2) by Gurney, Myers, and Podmore,[109] in 1886, who reported 4760 successes out of 17,653 trials in the guessing of suits, an excess of 347 over the most probable number, of which the probability that it was caused by chance alone was calculated by Edgeworth[110] to be .000,000,02, or, by another method of calculation,[111] .000,000,000,8; (3) by Miss B. Lindsay,[112] who reported that out of 976 trials in the guessing of six uncolored forms (figures on cards), there were made 198 successses, "the odds against obtaining that degree of success by chance being 1000 to 1," or, the probability of the success by chance, as calculated by Edgeworth,[113] being .002; (4) by Messrs. A. J. Shilton and G. T. Cashmore,[114] who reported that out of 505 trials in the guessing of red, blue, green, and yellow cards, 261 successes were made, the probability for an extra-chance cause being considered "a trillion trillions to 1," or, as calculated by Edgeworth,[115] .9999 [to 37 places]; (5) by the Misses Wingfield,[116] who made 2,624 trials in the guessing of two-place numbers (10-99), the cards being chosen at random by the agent, and reported 275 successes (not including 78 cases in which the right digits were guessed in reverse order) as against the most probable number of 29, the probability for extra-chance cause being "the ninth power of a trillion to 1"; (6) by Richet,[117] in France, in 1888-89 and 1889-90, who

[108] *Proceedings S. P. R.*, 1882-83, 1:70 ff.

[109] Phantasms of the Living, vol. I, p. 33.

[110] *Vide*, Podmore: Apparitions and Thought-Transference, 1894, p. 27.

[111] *Proceedings S. P. R.*, 4:203.

[112] Phantasms of the Living, 1:34.

[113] *Proceedings S. P. R.*, 4:203.

[114] Phantasms of the living, 1:34.

[115] *Proceedings S. P. R.*, 4:204.

[116] Phantasms of the Living, 1:34.

[117] La suggestion mentale et le calcul des probabilités. *Revue Philosophique*, 1884, 18:609 ff.

from 11 reagents got 789 right guesses on the suit out of 2927 trials, or 26.99% [118] as against the probability of 25%, or, if series of over 100 guesses are discarded because of the possible influence of fatigue, 510 out of 1833 trials, or 27.8%;[119] and from 782 trials, 17 right guesses on the complete card, or 2.17%, as against 1.92%; (7) by Mr. and Mrs. Brown,[120] in the United States, in 1889, who in 1000 guesses on the number of the card got 21.9% right as against 10%. Negative results have been reported by the Committee on Thought-Transference[121] of the American Society for Psychical Research, in 1885-89, who obtained from 5500 guesses on the color of the card, by 22 reagents, 50.51% right cases, while an experimental chance series of 5150 trials yielded 50.35%, and probability is 50%; and from 4000 guesses on the number of the card, 10.3% right cases as against a probability of 10%. Negative results seem to have been encountered in considerable numbers by the (English) Society for Psychical Research also, but, unfortunately, were not considered worthy of public record.[122]

"Card-experiments of the above type offer special conveniences for the very extended trials which we wish to see carried out: they are easily made and rapidly recorded." [123]

Cards were turned to by Richet [124] because they permitted a quantitative calculation of results, and were used by the American Committee

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[118] For this result the probability has been calculated by Edgeworth to be .008 (*Proceedings S. P. R.*, 4: 202).

[119] For this result Edgeworth calculates the probability to be less than .0013. (*Ibid.*, p. 203).

[120] *Proceedings Am. S. P. R.*, Series 1, 1: 322-349.

[121] *Idem*, pp. 6 ff.

[122] *Vide*, Quotations from the *Journal S. P. R.*, on p. 23, and from Thomas on p. 26. Where *successful* results alone are regarded as *interesting*, the published results cannot be taken as indicative of the extent to which the telepathic capacity is shared by people in general. The situation becomes still more critical for the telepathic hypothesis when psychical-research writers are over-ready to impute improper experimental procedure to negative results, as Podmore appears to do in his reference to the experiments carried out under the direction of the American Society for Psychical Research: "But in the absence of details as to the conditions under which the experiments were made, no unfavorable inference can fairly be drawn from these results." (Apparitions and Thought-Transference, p. 27). Our inductions will be safer if *all* of the results of investigations which we have reason to believe have been intelligently carried out are available for our examination.

[123] Phantasms of the Living, 1: 34.

[124] For his later preference for playing-cards, *vide*, *Proceedings S. P. R.*, 1889, 6: 66.

for the same reason. The numbers of experiments from these two more authoritative sources are extensive, but further work by others, of a similar kind, was strongly urged by Richet for the purpose of substantiating or subverting the results already at hand; and although many of the experiments carried out under the direction of the American Committee were conducted by men of science, other experiments under the control customary in the modern psychological laboratory are desirable to determine whether their negative results or Richet's slightly favorable results [125] shall be expected at large. And playing cards possess obvious advantages: as apparatus they are easily accessible; they are convenient to shuffle, cut, and draw by chance; they permit testing for the relative preference [126] of Thought-Transference for Color, Number, Form and Internal Speech; and they offer convenient chance series of 1:2, 1:4, 1:10, and 1:40 (if the face-cards, or court-cards, are discarded).

In order to test the hypothesis of a common "Suggestion Mentale"; to analyze out the conditions of experimentation responsible for success, if found; to make a psychological study of the mental processes of the reagent in the thought-transference situation; to get material with which to make a comparison between inductive and theoretical probability; and to establish a "norm" for a definite test for thought-transference or clairvoyance: this investigation, continuing through four years [127] and involving over 110 sets of 100 experiments each, was made.

Reagents.

Our research was conducted with the assistance of 105 reagents (percipients) and 97 experimenters (agents), (the writer acting as experimenter in 18 sets of experiments). All assisting experimenters and reagents were students in the general lecture course in psychology or were doing laboratory work in the department, or both; were in their first (29), second (72), third (50), fourth (42), or fifth (7) year of university work; were pursuing their major subjects of study in twenty

[125] Preyer (Die Erklärung des Gedankenlesens, Leipzig, 1886, pp. 48 ff) challenges Richet's results upon which he announced "Suggestion mentale" as a slight but normal human phenomenon, claiming that the slight excess over probability is within the limits of chance, and that Richet also discarded unfavorable series.

[126] *Cf.*, Reference to such preference by the American Committee on Thought-Transference, *Proceedings Am. S. P. R.*, Series 1, 1:9, 45, 106, 111.

[127] 1912-1916.

different departments of the university; and had registered from twenty-one different states and from various sections of California. They were thus a fairly representative group of normal people. Their attitude toward the telepathic hypothesis determined their selection from the class for this research. It was generally that of positive belief in it. Of the reagents, most of them expressed confidence, based upon their own experience, either in "The feeling of being stared at" (40), or in the power to "Will another to turn around" (1), or in both (45), or in thought-transference (11); the few undecided or agnostic ones (4) were open-minded and were willing to give the hypothesis a fair trial. Of the experimenters, most of them likewise expressed their faith, based upon their own experience, in the powers of "The feeling of being stared at" (10), or of "Willing" (7), or of both (59), or in thought-transference (5); again the few undecided or agnostic ones (10) were open-minded and were willing to do their best to give the hypothesis a fair trial. Apart from their own experience, almost all of both groups, including some of the uncertain and agnostic ones, have "convincing knowledge" of thought-transference experienced in their families or among their relatives, friends, or acquaintances. As will be shown later, the experience of the four or five days' work on a set of 100 experiments more often than not left the faith whole and the reagent expectant of statistical proof.[128]

Method.

In order to determine whether transferred knowledge, if any should be found, involved the use of visual imagery, as appeared to be the case in most of the English experiments, or of kinaesthetic imagery (incipient pronouncing) which Hansen & Lehmann[129] found to be effective in the guessing of numbers, the card drawn by the experimenter was held in his mind in three different forms of content, the form being determined for each experiment by the casting of an odd number of a die: *1*, visual impression; *3*, kinaesthetic image (stripped of auditory accompaniment); *5*, combined visual impression, kinaesthetic image, and auditory image. In the first form the upper left-hand corner of the card was critically inspected, visually defining as vividly as possible the small Arabic numeral in the corner and the color and form of the pip which

[128] *Vide, infra,* pp. 109 ff.

[129] *Philosophische Studien,* 1895, 11:471-530.

determined the suit, and since the rest of the card was not screened, the general form, number, and distribution of the pips on the card were represented in non-focal consciousness; in the second form, the corner of the card was quickly glanced at, then concealed and named in inner speech (as "Five of Diamonds") with consciousness vividly focused upon the "feel" of the imaged movement of the vocal organs, abstracting entirely from the image of the sound; in the third form, the first and second were combined and an auditory image of the experimenter's own voice in unison with the voices of others shouting the name of the card; the last form involved some shifting of the attention, but it was perhaps the most vivid of the three. Besides the cognitive mental content, the experimenter held in consciousness a determined attitude of will that the content should reach the reagent.

In order to have an experimental probability with which to compare results, or to determine definitely that phenomena of thought-transference, if found, are distinct from phenomena of lucidity, if found, blank or control experiments were provided for in every series, and were conditioned by an even number of spots on the die. This provision enables us to avoid certain defects in the entirely separate series of control experiments.

To determine, within our limits, the influence of distance between reagent and experimenter, the experimenter, in the first 3000 experiments, moved his chair every 20 experiments, over the following positions: 1, 2, 3, 4.6, 6, and 10 meters. And to determine the influence of the length of the "interval of impression," the time was varied, in another 2500 experiments, over 20, 40, and 60 seconds, giving the reagent the privilege, after the first three or five series of his set, of choosing his optimal time

Guesses were recorded by the reagent upon the color, number, and suit, of the card separately. Besides impelling him to inspect his impressions more critically, this device permitted the calculation which would determine whether color or form is more transferable. The record was kept covered by a sheet upon which introspections for each experiment were noted. The latter were tabulated according to the following headings: (1) Was the mind in a good receptive mood? (2) With respect to the imagery in which your impression came, what was its (*a*) Kind (visual, auditory, kinaesthetic)? (*b*) Vividness (Grade *A* to *D*, *A* for very vivid)? (*c*) Temporal course (did it come at the beginning, middle, or end, of the interval; did it come quickly—flash

out—or did it develop slowly; was it intermittent, persistent, or fleeting)? (*d*) Spatial attribute (where do you image the card, at your back, front, right, left, and how many meters away; or is it in your head)? (3) What is the certainty of your judgment?[130] (*a*) Grade *A* to *D*, *A* for a very good chance of its being right; *D* for only a little better chance than a pure guess would have. (*b*) If you graded your guess above *C*, what was there in your experience upon which you rely for your confidence? The following introspections taken from the record will illustrate:

Reagent	Experiment	*1*	*2*				*3*	
			a	*b*	*c*	*d*	*a*	*b*
4	15	Yes	V	B	bp	f1	B	Came Immediately and was very persistent.
13	59	Yes	V	B	bq	f½	B	Saw it distinctly.

For Reagent *4*, in his 15th experiment, the mind was in a good receptive condition during the interval of impression; the impression of a card came in visual imagery (V), vividly (B), at the beginning (b) of the interval, and was persistent (p); the card was imaged in front (f), about 1 meter distant; the guess was made with a high degree of certainty (grade B) that it must be right because the impression "Came immediately and was very persistent." The introspections of Reagent *13*, for his 59th experiment, are similarly interpreted; except that his impression came quickly (q), and was not noted as persistent; the card was imaged in front about ½ a meter distant; and the guess was given a high grade of certainty because the reagent "Saw it distinctly." (Facsimiles of the ruled forms for recording introspections and for recording guesses may be seen in Appendix B.)

The experimenter with a watch before him, (1) shuffles the deck of 40 playing cards (the face cards being discarded),[131] cuts the pack, and holds cards concealed; (2) shakes the dice-box, to determine a control or regular experiment, and, if the latter, the form of content the card is to have in his mind; (3) if a regular experiment, he turns over the pack, exposing to his view the under card, taps once to signal the reagent that the experimental period begins, holds mental con-

[130] For reference to the desirability of noting the relation of confidence of judgment to success, *vide, Proceedings Am. S. P. R.*, Series 1, 1:109, 262.

[131] *Cf.*, Instructions by the American Committee on Thought-Transference, *Proceedings Am. S. P. R.*, Series 1, 1:46, 262.

tent of card and wills the content to be projected into the mind of the reagent, and, after 15 to 20 or more seconds [132] taps twice to signal the close of the interval. After he notes that the reagent has recorded his guess, and has turned to his introspections, he records the color, number, and suit of the card and the number of the die-spot which conditioned the form of the experiment (as, *R5H 1*, for Red, Five of Hearts, Die-spot 1—*i. e.*, held in Visual Impression). The control experiments ran off in precisely the same form as the regular, except that the card remained unknown until the reagent had recorded his guess.

Good experimental conditions [133] were maintained: quiet, no conversation, regularity of procedure, etc. The reagent sat with his back toward the experimenter, and in the experimental interval he closed his eyes, "thought of nothing," and assumed a calm, receptive, quietly expectant state of mind; he was cognizant of the method employed by the experimenter and held himself ready to receive impressions in any sense mode; he was given all the time he needed to note down his introspections, which in the early series of the set ran the experiments at the rate of one in 5-10 minutes, but later permitted a higher rate, one a minute as a maximum. Experiments were not made after fatigue point had been reached; a set of 10 series of 10 experiments each, 100 experiments, taking the reagent three to five days (one week apart, and at the same hour of the day) to complete.

The records were not compared by the reagent or the experimenter before the set was complete [134] and but seldom after: the procedure was "without knowledge," and precautions were taken to avoid the rise of any rumor (such as "Our results show no telepathy") which might influence those still engaged in the experiments.

Results.

Table XIII (on the verso page) gives (1) the number of the reagent, (2) the number of correct guesses upon (*a*) the whole card, (*b*)

[132] *Vide, supra,* p. 52.

[133] For what are considered conditions essential to success, *vide,* Schmoll and Mabire: Experiments in thought-transference (tr. from the French), *Proceedings S. P. R.,* 1888, 5:205; also, Report of the American Committee on Thought-Transference, June 15, 1886, *Proceedings Am. S. P. R.,* Series 1, 1:110-111; *idem,* May 1887, p. 261.

[134] *Cf.,* Gurney, Myers, and Podmore: Phantasms of the Living, vol. I, p. 33, foot-note.

the color, (*c*) the number of spots, (*d*) the suit, (3) the number of guesses wholly wrong, and (4) the total number of experiments. The "Card Not Imaged" side of the table gives the data of the control experiments, *i. e.*, of guesses upon cards unknown to the experimenter.

Table XIV (on the recto page) gives the deviations from approximate probability, and was derived from the data of Table XIII after the latter was reduced to sets of 50 experiments.[135] In Table XIII, for example, Reagent 1 made 26 R guesses on color in 55 control experiments, and 22 in 45 regular experiments; reduction of the control and regular experiments to 50 each makes the value of the R guesses 24 in each case (disregarding decimals), which is a deviation of −1 from the "probable number" 25. The deviations of guesses on Card, and Suit, and of guesses Wholly Wrong, are made from the integral number nearest the value of the "probable number," which in these cases is fractional; *i. e.*, on the Card it is made from 1 instead of from 1.25; on the Suit from 12 if minus, or 13 if plus, instead of from 12.5; etc.[136] This is the only table in which the results of the different reagents may be compared directly.

Table XV gives the totals per 1000; Table XVI gives (1) the per cent of R cases (*a*) per 1000 experiments, and (*b*) the per cent for the total, as well as (2) the per cent expected from theoretical probability; and Table XVII gives the deviations from the probable per cent.

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[135] This reduced table (Table XIII*a*) may be seen in Appendix A.

[136] The reader will readily recognize the fact that these deviations are only approximate and, consequently, cannot be compared for slight differences. In the reduction to sets of 50, the value found is an integer which may vary in value from −0.5 to +0.4. Deviation is calculated from the integer. In no case, however, can the neglected values exceed a deviation of ±0.5 in the cases of Colors or Numbers, or ±1. in the cases of Suits or Wrong guesses, or ±0.75 in the case of Cards. The justification for neglecting these fractional deviations lies in their small value as compared with the deviations themselves, and in the increased clearness of the table consequent upon the dispensing with decimals. The smaller differences, moreover, are not significant in sets of 50 experiments; they reach significance only in the averages, or in per cents calculated from the averages, of a number of sets, in which statistical consideration we turn to the original unreduced data.

TABLE XIII. NUMBER OF RIGHT CASES IN THE GUESSING OF PLAYING-CARDS.

(Sets of 100 Experiments.)

A. Normal Persons (Students).

	Card Not Imaged						Card Imaged					
Reagent	Card	Color	No.	Suit	W	Total	Card	Color	No.	Suit	W	Total
1.	4	26	6	16	28	55	3	22	7	11	21	45
2.	3	26	5	14	19	46	0	20	6	7	30	54
3.	2	36	5	19	10	47	2	28	5	12	24	53
4.	2	21	5	13	22	46	2	28	8	13	21	54
5.	2	25	6	10	25	54	1	17	3	10	27	46
6.	2	31	6	13	17	52	0	26	4	10	20	48
7.	1	18	3	12	29	48	2	29	3	13	23	52
8.	0	28	5	14	24	55	0	18	2	12	25	45
9.	0	23	3	10	18	44	1	24	4	14	29	56
10.	2	31	4	16	23	56	1	23	4	14	18	44
Totals 1st 1000	18	265	48	137	215	503	12	235	46	116	238	497
11.	1	25	8	14	20	51	2	25	8	12	18	49
12.	1	24	7	12	17	47	1	31	8	16	20	53
13.	2	21	7	8	29	53	1	17	4	7	28	47
14.	0	29	3	12	24	55	1	19	5	8	22	45
15.	1	24	4	10	17	43	0	29	5	16	26	57
16.	1	23	4	14	25	50	1	29	6	15	19	50
17.	3	26	6	11	24	52	1	24	4	18	21	48
18.	1	20	6	8	20	43	1	22	2	12	34	57
19.	2	21	8	14	16	42	2	28	7	13	26	58
20.	0	27	7	12	24	56	1	25	4	13	18	44
Totals 2d 1000	12	240	60	115	216	492	11	249	53	130	232	508
21.	1	18	2	6	28	46	2	22	6	13	28	54
22.	0	26	1	11	15	41	1	23	4	8	33	59
23.	0	23	1	15	20	44	2	24	6	12	28	56
24.	0	27	7	12	18	47	1	28	6	16	22	53
25.	4	31	9	20	18	52	4	28	11	15	17	48
26.	0	28	1	19	22	51	4	25	9	13	21	49
27.	0	15	1	9	27	42	2	31	8	18	21	58
28.	2	22	3	14	25	48	1	26	5	15	23	52
29.	0	21	2	13	22	44	0	31	5	8	22	56
30.	0	18	1	9	25	44	3	28	11	17	23	56
Totals 3d 1000	7	229	28	128	220	459	20	266	71	135	238	541

TABLE XIV. RIGHT CASES IN THE GUESSING OF PLAYING-CARDS. DEVIATIONS FROM APPROXIMATE PROBABILITY.

A. Normal Persons (Students).

			Card Not Imaged					Card Imaged				
			Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
Probable No.			1.25	25	5	12.5	22.5	1.25	25	5	12.5	22.5
		Reagent.										
1st	1000	*1.*	+3	−1	0	+2	+2	+2	−1	+3	0	0
		2.	+2	+3	0	+2	−1	−1	−6	+1	−6	+5
		3.	+1	+13	0	+7	−11	+1	+1	0	−1	0
		4.	+1	−2	0	+1	+1	+1	+1	+2	0	−3
		5.	+1	−2	+1	−3	0	0	−7	−2	−1	+6
		6.	+1	+5	+1	0	−6	−1	+2	−1	−2	−1
		7.	0	−6	−2	0	+7	+1	+3	−2	0	0
		8.	−1	0	0	0	0	−1	−5	−3	0	+5
		9.	−1	+1	−2	−1	−2	0	−4	−1	0	+3
		10.	+1	+3	−1	+1	−1	0	+1	0	+3	−2
2d	1000	*11.*	0	0	+3	+1	−2	+1	+1	+3	0	−4
		12.	0	+1	+2	0	−4	0	+4	+3	+2	−3
		13.	+1	−5	+2	−4	+4	0	−7	−1	−5	+7
		14.	−1	+1	−2	−1	0	0	−4	+1	−3	+1
		15.	0	+3	0	0	−2	−1	0	−1	+1	0
		16.	0	−2	−1	+1	+2	0	+4	+1	+2	−3
		17.	+2	0	+1	−1	0	0	0	−1	+6	0
		18.	0	−2	+2	−3	0	0	−6	−3	−1	+7
		19.	+1	0	+5	+4	−3	+1	−1	+1	−1	0
		20.	−1	−1	+1	−1	−1	0	+3	0	+2	−2
3d	1000	*21.*	0	−5	−3	−5	+7	+1	−5	+1	0	+3
		22.	−1	+7	−4	0	−4	0	−6	−2	−5	+5
		23.	−1	+1	−4	+4	0	+1	−4	0	−1	+2
		24.	−1	+4	+2	0	−3	0	+1	+1	+2	−1
		25.	+3	+5	+4	+6	−5	+3	+4	+6	+3	−4
		26.	−1	+2	−4	+6	0	+3	+1	+4	0	−1
		27.	−1	−7	−4	−1	+9	+1	+2	+2	+2	−4
		28.	+1	−2	−2	+2	+3	0	0	0	+1	0
		29.	−1	−1	−3	+2	+2	−1	+3	−1	−5	−2
		30.	−1	−5	−4	−2	+5	+2	0	+5	+2	−1

TABLE XIII—Continued.

Reagent	Card Not Imaged						Card Imaged					
	Card	Color	No.	Suit	W	Total	Card	Color	No.	Suit	W	Total
31.	0	14	3	7	22	38	2	36	7	20	23	62
32.	5	25	8	17	25	51	6	26	10	20	20	49
33.	2	31	5	18	18	52	2	31	5	19	17	48
34.	2	24	4	15	20	46	1	34	7	15	16	54
35.	1	24	3	15	23	48	0	28	3	12	23	52
36.	1	28	7	14	15	46	2	31	5	18	23	54
37.	2	39	6	23	15	56	0	18	5	10	22	44
38.	0	21	4	10	29	53	2	24	4	9	22	47
39.	0	19	2	10	27	48	1	26	3	15	25	52
40.	0	21	1	10	23	44	4	30	7	18	25	56
Totals 4th 1000	13	246	43	139	217	482	20	284	56	156	216	518
41.	1	31	4	11	18	52	3	24	9	10	20	48
42.	1	24	2	13	24	49	3	26	10	12	19	51
43.	2	31	4	12	17	48	0	25	4	13	24	52
44.	0	26	4	13	18	48	1	25	7	13	22	52
45.	1	23	6	10	22	48	0	30	3	16	20	52
46.	2	31	8	12	17	53	2	20	4	12	26	47
47.	0	28	1	14	22	50	2	28	8	15	17	50
48.	1	31	6	13	23	57	0	23	4	13	19	43
49.	1	23	7	10	16	43	5	29	12	19	25	57
50.	1	24	3	9	23	48	1	28	5	13	21	52
Totals 5th 1000	10	272	45	117	200	496	17	258	66	136	213	504
51.	3	15	5	10	21	37	2	28	8	16	31	63
52.	3	28	4	11	15	43	2	30	8	20	24	57
53.	1	26	6	12	23	53	1	24	5	11	19	47
54.	2	23	7	15	23	50	1	23	4	13	24	50
55.	2	26	5	11	27	56	1	17	2	9	26	44
56.	0	29	8	10	23	56	2	28	4	16	14	44
57.	2	18	5	6	25	46	3	23	7	15	27	54
58.	1	26	9	11	17	48	1	22	5	12	27	52
59.	1	31	4	18	11	43	4	30	9	13	23	57
60.	1	30	5	14	22	56	2	23	6	13	19	44
Totals 6th 1000	16	252	58	118	207	488	19	248	58	138	234	512

TABLE XIV—Continued.

		Card Not Imaged					Card Imaged				
		Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
Probable No.		1.25	25	5	12.5	22.5	1.25	25	5	12.5	22.5
	Reagent										
4th 1000	*31.*	−1	−7	−1	−3	+6	+1	+4	+1	+3	−3
	32.	+4	0	+3	+4	+2	+5	+2	+5	+7	−2
	33.	+1	+5	0	+4	−5	+1	+7	0	+7	−4
	34.	+1	+1	−1	+3	0	0	+6	+1	+1	−7
	35.	0	0	−2	+3	+1	−1	+2	−2	0	0
	36.	0	+5	+3	+2	−6	+1	+4	0	+4	−1
	37.	+1	+10	0	+8	−9	−1	−5	+1	−1	+2
	38.	−1	−5	−1	−3	+4	+1	+1	−1	−2	0
	39.	−1	−5	−3	−2	+5	0	0	−2	+1	+1
	40.	−1	−1	−4	−1	+3	+3	+2	+1	+3	0
5th 1000	*41.*	0	+5	−1	−1	−5	+2	0	+4	−2	−1
	42.	0	−1	−3	0	+1	+2	0	+5	0	−3
	43.	+1	+7	−1	0	−4	−1	−1	−1	0	0
	44.	−1	+2	−1	+1	−3	0	−1	+2	0	−1
	45.	0	−1	+1	−2	0	−1	+4	−2	+2	−3
	46.	+1	+4	+3	−1	−6	+1	−4	−1	0	+5
	47.	−1	+3	−4	+1	0	+1	+3	+3	+2	−5
	48.	0	+2	0	−1	−2	−1	+2	0	+2	0
	49.	0	+2	+3	0	−3	+3	0	+6	+4	0
	50.	0	0	−2	−3	+1	0	+2	0	0	−2
6th 1000	*51.*	+3	−5	+2	+1	+5	+1	−3	+1	0	+2
	52.	+2	+8	0	0	−5	+1	+1	+2	+5	−1
	53.	0	0	+1	−1	0	0	+1	0	0	−2
	54.	+1	−2	+2	+2	0	0	−2	−1	0	+1
	55.	+1	−2	−1	−2	+1	0	−6	−3	−2	+7
	56.	−1	+1	+2	−3	−1	+1	+7	0	+5	−6
	57.	+1	−5	0	−5	+4	+2	−4	+1	+1	+2
	58.	0	+2	+4	−1	−4	0	−4	0	0	+3
	59.	0	+11	0	+8	−9	+3	+1	+3	−1	−2
	60.	0	+2	−1	0	−2	+1	+1	+2	+2	0

TABLE XIII—Continued.

Reagent	Card Not Imaged						Card Imaged					
	Card	Color	No.	Suit	W	Total	Card	Color	No.	Suit	W	Total
61.	1	23	5	13	18	45	0	24	0	13	31	55
62.	1	26	2	11	17	43	0	33	5	10	22	57
63.	2	24	6	15	20	46	4	27	6	20	25	54
64.	6	30	9	17	18	49	3	27	7	14	24	51
65.	2	26	7	13	26	55	1	22	3	10	21	45
66.	3	27	9	11	26	53	3	23	10	14	19	47
67.	2	28	6	16	18	48	0	19	4	13	29	52
68.	2	23	7	9	19	46	2	29	5	16	24	54
69.	2	24	3	15	23	48	2	27	5	13	23	52
70.	2	27	8	13	23	53	2	17	5	10	27	47
Totals 7th 1000	23	258	62	133	208	486	17	248	50	133	245	514
71.	3	30	3	16	30	60	1	17	4	11	22	40
72.	2	20	5	12	23	45	0	27	3	9	27	55
73.	1	24	4	16	16	42	1	23	7	10	30	58
74.	2	21	4	11	13	36	0	37	3	19	25	64
75.	0	31	9	14	18	55	2	21	5	13	22	45
76.	3	28	10	13	19	50	1	25	4	12	23	50
77.	1	33	6	15	21	57	2	15	6	8	24	43
78.	2	32	4	20	25	59	4	19	6	12	21	41
79.	0	16	5	9	31	50	2	26	3	13	23	50
80.	0	27	3	11	21	50	0	26	3	14	21	50
Totals 8th 1000	14	262	53	137	217	504	13	236	44	121	238	496
81.	1	21	3	9	30	52	1	25	4	12	20	48
82.	0	23	3	15	17	43	1	30	4	15	26	57
83.	2	25	5	13	22	48	1	24	6	12	24	52
84.	2	28	8	12	17	50	1	29	6	14	19	50
85.	1	18	4	9	30	50	1	26	5	11	20	50
86.	1	33	4	14	18	53	3	28	6	16	16	47
87.	0	18	3	6	17	38	1	28	7	13	30	62
88.	2	36	4	15	19	56	0	20	2	11	24	44
89.	0	29	4	14	16	47	2	24	3	11	28	53
90.	1	24	6	13	30	57	1	21	6	9	17	43
Totals 9th 1000	10	255	44	120	216	494	12	255	49	124	224	506

TABLE XIV—Continued.

Probable No.	Reagent	Card Not Imaged Card 1.25	Color 25	No. 5	Suit 12.5	W 22.5	Card Imaged Card 1.25	Color 25	No. 5	Suit 12.5	W 22.5
7th 1000	*61.*	0	+1	+1	+1	−2	−1	−3	−5	0	+5
	62.	0	+5	−3	0	−2	−1	+4	−1	−3	−3
	63.	+1	+1	+2	+3	0	+3	+0	+1	+6	0
	64.	+5	+6	+4	+4	−4	+2	+1	+2	+1	+1
	65.	+1	−1	+1	0	+1	0	−1	−2	−1	0
	66.	+2	0	+3	−2	+2	+2	−1	+6	+2	−2
	67.	+1	+4	+1	+4	−3	−1	−7	−1	0	+5
	68.	+1	0	+3	−2	−1	+1	+2	0	+2	0
	69.	+1	0	−2	+3	+1	+1	+1	0	0	0
	70.	+1	0	+3	0	0	+1	−7	0	−1	+6
8th 1000	*71.*	+1	0	−3	0	+2	0	−4	0	+1	+5
	72.	+1	−3	+1	0	+3	−1	0	−2	−4	+2
	73.	0	+4	0	+6	−3	0	−5	+1	−3	+3
	74.	+2	+4	+1	+2	−4	−1	+4	−3	+2	−2
	75.	−1	+3	+3	0	−6	+1	−2	+1	+1	+1
	76.	+2	+3	+5	0	−3	0	0	−1	0	0
	77.	0	+4	0	0	−4	+1	−8	+2	−3	+5
	78.	+1	+2	−2	+4	−1	+4	−2	+2	+2	+3
	79.	−1	−9	0	−3	+8	+1	+1	−2	0	0
	80.	−1	+2	−2	−1	−1	−1	+1	−2	+1	−1
9th 1000	*81.*	0	−5	−2	−3	+6	0	+1	−1	0	−1
	82.	−1	+2	−2	+4	−2	0	+1	−1	0	0
	83.	+1	+1	0	+1	0	0	−2	+1	0	0
	84.	+1	+3	+3	0	−5	0	+4	+1	+1	−3
	85.	0	−7	−1	−3	+7	0	+1	0	−1	−2
	86.	0	+6	−1	0	−5	+2	+5	+1	+4	−5
	87.	−1	−1	−1	−4	0	0	−2	+1	−2	+1
	88.	+1	+7	−1	0	−5	−1	−2	−3	0	+4
	89.	−1	+6	−1	+2	−5	+1	−2	−2	−2	+3
	90.	0	−4	0	−1	+3	0	−1	+2	−2	−2

TABLE XIII—Continued.

Reagent	Card Not Imaged						Card Imaged					
	Card	Color	No.	Suit	W	Total	Card	Color	No.	Suit	W	Total
91.	2	22	4	12	18	42	1	35	3	20	21	58
92.	1	21	5	11	26	48	1	21	8	12	25	52
93.	3	21	5	8	18	41	1	29	5	15	28	59
94.	1	24	4	13	19	44	0	33	1	15	23	56
95.	1	15	5	9	22	40	1	25	7	19	30	60
96.	1	24	5	7	31	55	0	26	2	14	18	45
97.	2	24	3	11	26	50	4	24	4	13	26	50
98.	1	16	4	11	22	40	2	27	6	11	31	60
99.	3	21	6	16	23	46	2	28	5	17	24	54
100.	3	24	6	10	28	55	0	29	4	19	14	45
Totals 10th 1000	18	212	47	108	233	461	12	277	45	155	240	539
B. "Psychics."*												
1.	0	23	8	8	23	51	2	26	5	13	20	49
2.	0	24	2	12	28	54	2	22	7	10	20	46
3.	3	29	6	15	18	49	4	27	6	14	24	51
4.	1	21	3	11	20	43	2	30	8	13	22	57
5.	0	24	2	16	20	46	2	31	4	19	22	54
Totals	4	121	21	62	109	243	12	136	30	69	108	257
6.	1	20	2	13	17	37	2	25	6	9	35	63
7.	3	29	8	18	22	55	0	28	4	14	15	45
8.	2	22	6	17	22	47	3	31	6	15	20	53
9.	0	17	2	6	19	38	0	28	6	16	28	62
10.	1	23	4	14	25	50	1	29	5	15	18	50
Totals	7	111	22	68	105	227	6	141	27	69	116	273
Grand Totals	11	232	43	130	214	470	18	277	57	138	224	530
C. Corneal Reflection.†												
I.	2	22	6	12	24	49	22	39	33	24	12	51
II.	1	20	9	11	19	45	33	46	37	38	9	55
III.	0	29	3	16	30	61	20	29	27	22	8	39
IV.	3	17	8	11	21	41	28	55	36	39	4	59
V.	1	30	6	14	15	49	9	33	19	16	15	51
Totals	7	118	32	64	109	245	112	202	152	139	48	255

* *Vide*, p. 125.

† *Vide*, p. 121.

TABLE XIV—Continued.

		Card Not Imaged					Card Imaged				
		Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
Probable	No.	1.25	25	5	12.5	22.5	1.25	25	5	12.5	22.5
	Reagent										
10th 1000	*91.*	+1	+1	0	+1	−1	0	+5	−2	+4	−4
	92.	0	−3	0	−1	+4	0	−5	+3	0	+1
	93.	+3	+1	+1	−2	0	0	0	−1	0	+1
	94.	0	+2	0	+2	0	−1	+4	−4	0	−1
	95.	0	−6	+1	−1	+5	0	−4	+1	+3	+2
	96.	0	−3	0	−6	+5	−1	+4	−3	+3	−2
	97.	+1	−1	−2	−1	+3	+3	−1	−1	0	+3
	98.	0	−5	0	+1	+5	+1	−2	0	−3	+3
	99.	+2	−2	+2	+4	+2	+1	+1	0	+3	0
	100.	+2	−3	0	−3	+2	−1	+7	−1	+8	−6
B. "Psychics."											
	1.	−1	−2	+3	−4	0	+1	+2	0	0	−2
	2.	−1	−3	−3	−1	+3	+1	−1	+3	−1	0
	3.	+2	+5	+1	+2	−4	+3	+1	+1	+1	+1
	4.	0	−1	−2	0	0	+1	+1	+2	−1	−3
	5.	−1	+1	−3	+4	0	+1	+4	−1	+5	−2
	6.	0	+2	−2	+5	0	+1	−5	0	−5	+5
	7.	+2	+1	+2	+3	−2	−1	+6	−1	+3	−5
	8.	+1	−2	+1	+5	0	+2	+4	+1	+1	−3
	9.	−1	−3	−2	−4	+2	−1	−2	0	0	0
	10.	0	−2	−1	+1	+2	0	+4	0	+2	−4
C. Corneal Reflection.											
	I.	0	−3	+5	0	−1	+29	+17	+29	+22	−14
	II.	+1	−3	+1	0	+1	+21	+13	+27	+11	−10
	III.	−1	−1	−3	0	+2	+25	+12	+30	+15	−12
	IV.	+3	−4	+5	0	+3	+23	+22	+26	+20	−19
	V.	0	+6	+1	+1	−7	+8	+7	+14	+3	−7

TABLE XV.

Totals (per 1000).

	Card Not Imaged						Card Imaged					
Students	Card	Color	No.	Suit	W	Total	Card	Color	No.	Suit	W	Total
1st 1000	18	265	48	137	215	503	12	235	46	116	238	497
2d 1000	12	240	60	115	216	492	11	249	53	130	232	508
3d 1000	7	229	28	128	220	459	20	266	71	135	238	541
4th 1000	13	246	43	139	217	482	20	284	56	156	216	518
5th 1000	10	272	45	117	200	496	17	258	66	136	213	504
6th 1000	16	252	58	118	207	488	19	248	58	138	234	512
7th 1000	23	258	62	133	208	486	17	248	50	133	245	514
8th 1000	14	262	53	137	217	504	13	236	44	121	238	496
9th 1000	10	255	44	120	216	494	12	255	49	124	224	506
10th 1000	18	212	47	108	233	461	12	277	45	155	240	539
Totals	141	2491	488	1252	2149	4865	153	2556	538	1344	2318	5135
Psychics	11	232	43	130	214	470	18	277	57	138	224	530

TABLE XVI.

Per Cent (per 1000).

	Card Not Imaged					Card Imaged				
Students	Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
1st 1000	3.58	52.7	9.5	27.2	42.7	2.41	47.3	9.2	23.3	47.9
2d 1000	2.44	48.8	12.2	23.4	43.9	2.16	49.0	10.4	25.6	45.6
3d 1000	1.52	49.8	6.1	27.9	47.9	3.70	49.2	13.1	25.0	44.0
4th 1000	2.70	51.0	8.9	28.8	45.0	3.86	54.8	10.8	30.1	41.7
5th 1000	2.02	54.8	9.1	23.6	40.3	3.37	51.2	13.1	27.0	42.2
6th 1000	3.28	51.6	11.9	24.2	42.4	3.71	48.4	11.3	27.0	45.7
7th 1000	4.73	53.1	12.7	27.3	42.8	3.30	48.2	9.7	25.8	47.3
8th 1000	2.78	51.9	10.5	27.1	43.1	2.62	47.5	8.9	24.3	48.0
9th 1000	2.03	51.6	8.9	24.3	43.7	2.37	50.4	9.7	24.5	44.3
10th 1000	3.90	46.0	10.2	23.4	50.6	2.23	51.4	8.9	28.8	44.5
Totals	2.90	51.2	10.0	25.8	44.1	2.98	49.7	10.5	26.2	45.1
Prob.	2.50	50.0	10.0	25.0	45.0	2.50	50.0	10.0	25.0	45.0
Psychics	2.34	49.3	9.1	27.6	45.5	3.40	52.3	10.8	26.0	42.3

TABLE XVII.

Deviations of per cents (per 1000) from the Probable per cent.

	Card Not Imaged					Card Imaged				
	Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
Probable %	**2.50**	**50**	**10**	**25**	**45**	**2.50**	**50**	**10**	**25**	**45**
Students										
1st 1000	+1.08	+2.7	−0.5	+2.2	−2.3	−0.09	−2.7	−0.8	−1.7	+2.9
2d 1000	−0.06	−1.2	+2.2	−1.6	−1.1	−0.34	−1.0	+0.4	+0.6	+0.6
3d 1000	−0.98	−0.2	−3.9	+2.9	+2.9	+1.20	−0.8	+3.1	0.0	−1.0
4th 1000	+0.20	+1.0	−1.1	+3.8	0.0	+1.36	+4.8	+0.8	+5.1	−3.3
5th 1000	−0.48	+4.8	−0.9	−1.4	−4.7	+0.87	+1.2	+3.1	+2.0	−2.8
6th 1000	+0.78	+1.6	+1.9	−0.8	−2.6	+1.21	−1.6	+1.3	+2.0	+0.7
7th 1000	+2.23	+3.1	+2.7	+2.3	−2.2	+0.80	−1.8	−0.3	+0.8	+2.3
8th 1000	+0.28	+1.9	+0.5	+2.1	−1.9	+0.12	−2.5	−1.1	−0.7	+3.0
9th 1000	−0.47	+1.6	−1.1	−0.7	−1.3	−0.13	+0.4	−0.3	−0.5	−0.7
10th 1000	+1.40	−4.0	+0.2	−1.6	+4.6	−0.27	+1.4	−1.1	+3.8	−0.5
Totals	+0.40	+1.2	0.0	+0.8	−0.9	+0.48	−0.3	+0.5	+1.2	+0.1
Psychics	−0.16	−0.7	−0.9	+2.6	+0.5	+0.90	+2.3	+0.8	+1.0	−2.7

Inspection of the data in Table XIII reveals great variation among the corresponding entries, which, of course, except in sets in which the die cast odd and even equally often, cannot be directly compared; and although this variation is slightly decreased by the reduction of each of the two parts of a set to a set of even 50 experiments, as is done in Table XIII*a* (in Appendix A), from which the deviations in Table XIV are calculated, the amount of variation in the "Total" column, which results from pure chance, warns us that we shall have to exercise great care in identifying "significant" deviations. Were we to disregard for a moment the possibility of a faculty of "lucidity" or clairvoyance, which might disturb apparently ostensibly chance results, and, with Richet,[137] consider guessing upon unknown cards a true "control" experiment yielding purely chance results, the deviations in Table XIV under "Card Not Imaged" stand before us as cases of chance deviation, beyond which "significant" values must vary if they are to be identified in the Table.

Glancing at the "Card Imaged" deviations in Table XIV, in search for "significant" values, our eye rests for a moment at Set *1*, of the 1st 1000 upon the +3 under "Number," but upon noting under "Number" on the "Card Not Imaged" side of the table the deviation of +3 (Set *11*, 2d

[137] *Revue Philosophique*, 1884, 18:617.

1000) and +5 (Set *19*, 2d 1000), we search for the largest chance deviation under Number, and find it to be ±5, of which we note there are six cases in the table; our "significant" deviation must be higher. We find +6 several times (Sets *25*, *49*, and *66*), but none higher until the last section (C) of the table is reached, where we see +14 to +30. Evidently we cannot be quite satisfied with the +6; especially since it occurs in only 3% of the sets. Searching thus for significant deviations, we find the largest deviations to be as follows:

TABLE XVIII.

Largest deviations in Table XIV.

Card Not Imaged					Card Imaged				
Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
+5	+13	+5	+8	−11	+5	+7	+6	+8	−7
−1	−9	−4	−6	+9	−1	−8	−5	−6	+7

In every case but one the positive deviations of the R guesses on the cards and the negative deviation of the wholly wrong guesses (W) are as large on the "Card Not Imaged" side of this table as upon the "Card Imaged" side.

Evidently, if we have any "significant" deviations in the first 100 Sets of Table XIV they are not to be discriminated from chance deviations by merely "inspecting" the table. We shall have to resort to statistical analysis.

Analysis of Results.

Suppose, for the sake of illustrating our method, some influence working for R cases has been present in our experiments, but was too slight to be revealed in Table XIV because the deviations incorporating it do not exceed some deviations known to be caused by chance alone—suppose this influence present, but masked by the large variations of chance, how can it be detected? It might be discovered by subjecting the data to various analyses which our method of experimentation permits, or to certain mathematical treatment already common in scientific investigation.

Let us give our attention first to the various analyses.

If the various ways of "imaging" the card have a varying effect upon the influence, the R cases (correct judgments or guesses) would fall disproportionately upon one or two of the die-spots, which, in the experiments, determined these favorable conditions; if the influence is ac-

companied by increased certainty in the consciousness of the reagent, R cases would fall more frequently upon the Highly Graded guesses than upon the others; if the distance between experimenter and reagent, or if the length of the interval of impression, is a factor of which the influence is a function, and these factors were varied in experimentation, appropriate analyses would show increased frequency of R cases upon the optimal distance or time; etc. In all these analyses the result would yield a deviation from the probable value larger than the deviations in Table XIV, and, perhaps, sufficiently large to be considered decisive.

The increasing feebleness of such an influence which might be smothered in the tables of totals or deviations and yet which might be detected by analysis would correspond to the increase in the number of experiments we choose to inspect, provided the influence is not confined to one or two sets, but is somewhat general: the greater the aggregate number of experiments under consideration, the more sensitive is our system of measurement, and the slighter may the influence be which can be decisively revealed.

Illustration.

To illustrate, we might inspect the deviations in the last set (*V*) in Table XIV. As many wholly wrong guesses were made in the "Card Imaged" experiments as in the "Card Not Imaged", and both, according to Table XIV, may be chance deviations. The deviations under Color (+7) and Suit (+3) are surpassed by chance deviations (+13 and +8); the deviation under Card (+8) is a little greater than that made by chance (+5). The deviation under Number alone (+14) greatly exceeds any of our chance deviations (+5). The apparently significant deviations then are limited to Card and Number. Are these really significant? And do the others mask some positive influence working for R cases?

If we distribute the R cases (correct judgments or guesses) over the die-spots which determined the conditions of the respective experiments in this set, and express their frequency in per cent, we get Table XIX. Table XX shows the deviations:

TABLE XIX.

Per Cent of R Cases.

Die-Spot Even.							Die-Spot Odd.						
Die-Spot	R Cases				W	Tot.	Die-Spot	R Cases				W	Tot.
	Card	Color	No.	Suit				Card	Color	No.	Suit		
Probable %	**2.5**	**50**	**10**	**25**	**45**			**2.5**	**50**	**10**	**25**	**45**	
2	6.3	56	31	31	25	16	*1*	33.3	89	56	44	6	18
4	0.0	67	0	27	27	15	*3*	5.0	35	15	15	60	20
6	0.0	56	6	28	39	18	*5*	15.4	77	46	38	15	13

TABLE XX.

Deviations

2	+3.8	+6	+21	+6	−20	*1*	+30.8	+39	+46	+19	−39
4	−2.5	+17	−10	+2	−18	*3*	+2.5	−15	+5	−10	+15
6	-2.5	+6	−4	+3	−6	*5*	+12.9	+27	+36	+13	−20

Upon inspecting Table XX we see immediately that under every head Die-spots *1* and *5* are favored. Moreover, although the number of experiments upon which the per cents are reckoned, as shown in the last column of Table XIX, is sufficiently small to permit large chance deviations, we must certainly recognize the deviations on Die-spot *1* as significant. If we feel slightly uncertain about the significance of the deviations on Die-spot *5*, we can suspend judgment until the results of other analyses are examined.

If we collect the R cases in the 11 guesses out of the 100 which were made with a greater degree of certainty than were the others, and calculate their per cent of frequency and reckon the deviations from the probable per cent, we get:

TABLE XXI.

Per cent of R Cases in High-Grade Guesses, Compared with per cent of R Cases in Guesses of all Grades.

	Card	Color	No.	Suit	W
Grade High	27	91	45	55	9
Probable %	2.5	50	10	25	45
Deviation*	+24.5	+41	+35	+30	−36

*These deviations can be made comparable to those in Table XIV by dividing them by two; which will permit a comparison, between the total Guesses (of the Imaged Experiments) and the guesses given a high grade of certainty:

	Card	Color	No.	Suit	W
All Grades	+8	+7	+14	+3	−7
Grade High	+13	+21	+18	+15	−18

And if we distribute them over the die-spots, we get:

TABLE XXII.

Even Spots					Odd Spots					
	R Cases			W Total		R Cases				W Total
Die	Card	Color	No.	Suit	Die	Card	Color	No.	Suit	
2		1		1	1	3	2		1	5
4		1		1	3					0
6				0	5		3	2	1	1 4
Totals		2	1	2		3	5	2	2	1 9

There can be no longer any serious doubt about the presence in this set of experiments, of some influence beyond chance working for R cases; it is operative in guessing upon the Card, Color, Number, and Suit; it is effective under the conditions of the experiment determined by Die-spots *1* and *5*; and it is associated with the reagent's feeling of certainty in making the guess. Analyses have magnified the deviations, some of which in Table XIV were not greater than chance deviations, have made them sufficiently conspicuous to be decisive, and have revealed some of the general characteristics of the influence thus detected.[138]

In a similar way we may set to work on the analyses of the first 100 sets of Table XIII. We provisionally disregard the possibility of the operation of a general faculty of "lucidity," which, conceivably, would be operative on the "Card Not Imaged" side of our Tables as well as on the "Card Imaged" side, and, assuming the presence of some real thought-transference which is so slight in amount as to be smothered in the chance deviations of Table XIV, we may expect to detect it, (*a*) if it varies in any way with the kinds of imagery in which the experimenter holds the card in his mind, (*b*) if it depends upon a correspondence between the form of imagery in which the impression comes to the reagent and the form of imagery in which the experimenter holds the card in his mind, or (*c*) if its functioning is correlated with higher certainty in the reagent's mind.

Relation Between R Cases and Experimenter's Imagery.

(*a*) If we distribute the R cases over the six die-spots, which controlled the conditions of the experiment, we shall get tables comparable to Tables XIX and XX, above, from which we may learn by search-

[138] For further treatment of these results, see *infra*, pp. 121 ff.

ing for unequal distribution whether thought-transference was a function of the imagery in the mind of the experimenter.

Table XXIII shows the distribution of R cases in 98 sets,[139] or 9800 experiments, over the die-spots. Table XXIV gives the per cents, and Table XXV the deviations of the latter from the probable per cents.

TABLE XXIII.

Number of R cases distributed over the die-spots (9800 Experiments).

	Card Not Imaged.							Card Imaged.						
Die-Spot	R Cases				W	Total		Die-Spot	R Cases				W	Total
	Card	Color	No.	Suit					Card	Color	No.	Suit		
2	47	805	162	394	703	1581		1	44	838	162	439	765	1681
4	54	819	167	411	656	1548		3	50	826	188	430	762	1682
6	38	813	146	424	751	1636		5	54	840	172	443	747	1672

TABLE XXIV.

Per Cent.

Prob. %	2.50	50	10	25	45	16.7		2.50	50	10	25	45	16.7
2	2.97	50.9	10.2	24.8	44.4	16.1	1	2.62	49.9	9.6	26.1	45.5	17.1
4	3.49	53.0	10.8	26.6	42.4	15.8	3	2.97	49.1	11.2	25.6	45.3	17.2
6	2.32	49.7	8.9	25.9	45.9	16.7	5	3.23	50.2	10.3	26.5	44.7	17.1

TABLE XXV.

Deviations from Probability.

2	+0.47	+0.9	+0.2	−0.2	−0.6	−0.6	1	+0.12	−0.1	−0.4	+1.1	+0.5	+0.4
4	+0.99	+3.0	+0.8	+1.6	−2.6	−0.9	3	+0.47	−0.9	+1.2	+0.6	+0.3	+0.5
6	−0.18	−0.3	−1.1	+0.9	+0.9	0.0	5	+0.73	+0.2	+0.3	+1.5	−0.3	+0.4

It is evident from Table XXV that, after segregation, the deviations are not noticeably magnified upon any one or two of the die-spots, as they were in Table XX, and consequently, the supposed influence is independent of the varying conditions of experimentation determined by the die-spots. Visual, auditory, and combined imagery in which the card is held in the experimenter's mind are equally indifferent to the influence. And since the positive deviations in the chance series (on the even die-spots) under Card, Color, and Suit, exceed those in the regular experiments (on the odd die-spots), and the largest positive deviation under Number (+1.2) is not large (as may be seen by comparison with the deviations on the "Card Not Imaged" side of Table XVII;[140]

[139] Two sets, in which the experimenters failed to discriminate between the even numbers, are omitted.

[140] *Supra*, p. 65.

the average of Sets *6*, *7*, and *8* there, the aggregate number of experiments of which is 1478, which is close enough to 1662 for comparison, we find to be +1.7), the supposed influence remains completely masked.

Relation Between R Cases and Congruity of Imagery.

(*b*) To determine whether our masked influence is dependent upon a correspondence between the mode of imagery in which the impression of a card comes into the consciousness of the reagent and the mode of imagery in which the card is held in the mind of the experimenter, and, if it is, to reveal the presence of this influence, we may, perhaps, limit ourselves to a tabulation of the R cases on the "Card," the most favorable case for thought-transference, according to the imagery of the reagent, over the die-spots which determined the experimenter's treatment of the card. This tabulation gives the following table, in which *V*, *K*, and *A*, stand for the modes of imagery, Visual, Kinasethetic, and Auditory, and *G* stands for "pure guess" made without any determining imagery.

TABLE XXVI.

R Cases on the "Card" according to the Imagery of the Reagent.
(100 sets of 100 experiments each.)

	Card Not Imaged						Card Imaged				
Die	*V*	*K*	*A*	*G*	Total	Die	*V*	*K*	*A*	*G*	Total
2	30	8	7	5	50	*1*	25	6	9	7	47
4	27	8	14	4	53	*3*	24	11	6	10	51
6	17	8	8	5	38	*5*	28	8	9	10	55
Totals	74	24	29	14	141		77	25	24	27	153

TABLE XXVII.

Per cent.

2	60.0	16.0	14.0	10.0	*1*	53.2	12.8	19.2	14.9
4	50.9	15.1	26.4	7.5	*3*	47.0	21.6	11.8	19.6
6	44.8	21.1	21.1	13.2	*5*	50.9	14.5	16.4	18.2
Totals	52.5	17.0	20.6	9.9		50.3	16.3	15.7	17.6

TABLE XXVIII.

Deviations of per cent from per cent of total cases.

2	+7.5	−1.0	−6.6	+0.1	*1*	+2.9	−3.5	+3.5	−2.7
4	−1.6	−1.9	+5.8	−2.4	*3*	−3.3	+5.3	−3.9	+2.0
6	−7.7	+4.1	+0.5	+3.3	*5*	+0.6	−1.8	+0.7	+0.6

In Table XXVI we find that the number of R guesses on the Card was 47 when the experiment was conditioned by Die-spot *1*, *i. e.*, when the card was held in the experimenter's mind in visual form, and that 25 of them were made from Visual imagery in the mind of the reagent. From Table XXVII we read that 53.2% of the correct guesses on cards held by the experimenter in visual form were made by the reagent from visual imagery; that 50.3% of all correct guesses on cards held by the experimenter in any form whatever were made from visual imagery. Table XXVIII shows this deviation in favor of correspondence of imagery as +2.9. Remembering that Die-spot *3* determined the Kinaesthetic form, and Die-spot *5* the Combined Visual-Kinaesthetic-Auditory form, the reader can see in the deviations some remarkably consistent, if slight, indications of a correspondence of imagery in the different forms: positive deviations of frequency of guesses from visual imagery upon Die-spots *1*, and *5*, the larger upon the former which determined the Visual form unmixed with other forms in the experimenter's mind; from the Kinaesthetic form upon Die-spot *3*, with greatest negative deviation upon Die-spot *1* which alone ruled out any Kinaesthetic form; from the Auditory form upon Die-spot *5*. The largest positive deviation for the "pure guesses" is found upon Die-spot *3*, the most difficult for the experimenter to entertain vividly in his mind. These deviations are remarkably consistent; only one entry is out of order and that is the +3.5 under Auditory guesses upon Die-Spot *1*, which, like Die-Spot *3*, determined imagery free from an auditory component, and which because of the greater ease in keeping it pure should have been correlated with a negative deviation even greater than the −3.9 upon Die-spot *3*. It is true, also, that there should have been a positive deviation under *K* upon Die-spot *5*, and, perhaps, a larger one under *V*, but these failings in terms of quantity of deviation are not serious. The serious question is whether this consistency in the evidence for a correspondence of imagery in the minds of experimenter and reagent rests upon deviations large enough to be significant, and is not merely a fortuitous coincidence among chance deviations. Glancing at the "Card Not Imaged" side of Table XXVIII we find even stronger evidence for an affinity of *V* for Die-spot *2*, of *K* for Die-spot *6*, and of *A* for Die-spot *4;* yet this is mere coincidence. Moreover, if we were to assign Visual imagery to Die-spot *2*, Kinaesthetic to Die-spot *6*, and Mixed imagery to Die-spot *4*, we should not only have stronger evidence, because of larger deviations, but we should have even greater consistency in the evidence for correspond-

ence between these hypothetical forms of imagery and the imagery of the reagent's impressions.

Our analysis here did not appreciably magnify deviations into values distinguishable from chance variation. If there is a correspondence between the form of imagery from which the reagent makes a successful guess upon a card and the form in which the card is represented in the experimenter's consciousness, it is too slight to be detected in these tables, and its support of our supposed influence for R cases is not available.

Relation Between R Cases and the Feeling of Certainty.

(*c*) To determine whether a supposed influence working for R cases is present in association with a feeling of certainty, on the part of the reagent, that his guess stands a much better chance of being right than a "pure guess" would stand, we can distribute the R guesses on the "Card" over the various grades, and compare their frequencies, and we can tabulate an equal number each of guesses given low grades and of guesses given high grades, count the R cases, and compare the two groups.

The distribution of the R guesses on the "Card" made by all reagents who graded guesses as high as *A* or *B* in the descending series of grades of certainty *A, B, C, D, Pure Guess,* is as follows:

TABLE XXIX.

R Cases on the "Card" Distributed Over Reagent's Grades of Certainty.
(10,000 Experiments, 4865+5135.)

	Card Not Imaged.						Card Imaged					
Grades	*A*	*B*	*C*	*D*	*Guess*	Total	*A*	*B*	*C*	*D*	*Guess*	Tot.
R Cases	8	27	50	41	15	141	5	27	62	33	26	153
Per Cent.	5.7	19.3	35.5	29.1	10.6		3.3	17.6	40.5	21.6	17.0	
Difference							−2.4	−1.7	+5.0	−7.5	+6.4	
Grades given the 10,000 guesses, %							3.9	14.0	37.1	26.2	16.3	*2.57

Or if we group the R cases about the medium grade,

	A + *B*	*C*	*D* + *Guess*	*A* + *B*	*C*	*D* + *Guess*
	25.0	35.5	39.7	20.9	40.5	38.6
Difference				−4.1	+5.0	−1.1

The per cents in this table show that R guesses on the "Card" are more frequently given low grades than high, and the deviations show that R guesses given high grades occur as frequently when the card is

* Undetermined.

unknown to the experimenter as when it is held vividly in his consciousness. The distribution of the grades approximates the complete distribution. There is no relation here between an influence working for successful guesses and the feeling of certainty that the guess stands a better chance of being right than a pure guess would stand.

Another tabulation can be made, however, which will test this relation further. Guesses given high grades (*A* or *B*) may be selected from every set in which they are found, and the R cases compared with the R cases in a selection of as many neighboring guesses given with low grades (*D* or *Pure Guess*). In order to make the selection of the data in a purely mechanical way, and to avoid a disproportionate contribution from any one or a few sets, the number of guesses of high grade and of low grade selected from each set was limited to 10 each, chosen immediately after the 25th experiment of the set, when the reagent should be at his best, or from any part of the set to complete the quota as nearly as the number of highly graded guesses in the set would permit; in every case the nearest guess with a low grade (preference given to "Pure Guess") was selected. Sets in which no guesses were given high grades were omitted.

The following table gives the number of R cases, number of Wrong cases, and the number of experiments, for both the guesses graded Low and the guesses graded High.

TABLE XXX.

R Cases in Guesses Graded Low and Graded High.

	Graded Low.						Graded High.					
Year	Card	Color	No.	Suit	W	Total	Card	Color	No.	Suit	W	Tot.
1912–13	5	103	22	49	104	200	10	120	32	73	84	220
1913–14	3	89	17	58	95	192	6	106	26	59	74	192
1914–15	8	110	24	62	110	229	13	111	29	63	108	229
1915–16	3	35	7	20	31	67	1	31	5	18	32	67
Totals	19	337	70	189	340	708	30	368	92	213	298	708

TABLE XXXI.

Per cent.

1912–13	2.27	46.8	10.0	22.3	47.3	4.55	54.5	14.5	33.2	38.2
1913–14	1.56	46.3	8.9	30.2	49.5	3.12	55.2	13.5	30.7	38.6
1914–15	3.49	48.0	10.5	27.1	48.0	5.68	48.5	12.7	27.5	47.2
1915–16	4.88	52.2	10.4	29.8	45.3	1.49	46.3	7.5	26.8	47.8
Totals	2.68	47.6	9.9	26.7	48.0	4.24	52.0	13.0	30.1	40.7

TABLE XXXII.

Deviations.

Year	Graded Low. Card	Color	No.	Suit	W	Graded High. Card	Color	No.	Suit	W
1912–13	−0.23	−3.2	0.0	−2.7	+2.3	+2.05	+4.5	+4.5	+8.2	−6.8
1913–14	−0.94	−3.7	−1.1	+5.2	+4.5	+0.62	+5.2	+3.5	+5.7	−6.4
1914–15	+0.99	−2.0	+0.5	+2.1	+3.0	+3.18	−1.5	+2.7	+2.5	+2.2
1915–16	+1.98	+2.2	+0.4	+4.8	+0.3	−1.01	−3.7	−2.5	+1.8	+2.8
Totals	+0.18	−2.4	−0.1	+1.7	+3.0	+1.54	+2.0	+3.0	+5.1	−4.3

The amounts of these deviations do not appear beyond chance variation when the number of cases is considered, since they can be duplicated by averages of four entries in Table XIV under "Card Not Imaged"; but the consistency of the positive deviations appears significant: in all cases, except four, the deviations of R cases under "Grade High" are positive and are higher than the few corresponding positive deviations under "Grade Low." To be sure that a supposed influence for R cases is shown here in correlation with a feeling of certainty, we must ascertain whether this consistency of positive deviations may be fortuitous. Might it be matched by another sampling of an equal number of R cases on "Grades Low"? The deviations of an additional sampling are shown in

TABLE XXXIII.

Deviations.

Graded Low.

Year	Card	Color	No.	Suit	W	Total
1912–13	−1.14	+1.8	−1.4	−5.0	−1.4	220
1913–14	−1.98	+4.7	−4.3	0.0	−3.3	192
1914–15	+0.12	−2.8	0.0	−0.6	+2.6	229
1915–16	−1.01	−0.7	+3.4	−1.1	+1.3	67
Totals	−0.95	+0.9	−1.2	−1.8	−0.4	708

So far as this table gives testimony, the apparent significance noted above is increased. Further analysis, however, is competent to magnify those deviations if they really signify the presence of an influence working for R cases. The supposed influence would be operative on "Cards Imaged" alone and could be detected by re-tabulating all of the R cases of the 708 guesses given with a high grade of certainty, in such a way as to segregate the regular from the control experiments, as is shown below:

XXXIV.

Guesses Given High Grades.

	Card Not Imaged						Card Imaged					
		R Cases			W	Tot.		R Cases			W	Tot.
Year	Card	Color	No.	Suit			Card	Color	No.	Suit		
1912–13	4	54	17	30	39	104	6	66	15	43	45	116
1913–14	3	56	10	26	34	94	3	50	16	33	40	98
1914–15	6	49	17	27	48	102	7	62	12	36	60	127
1915–16	1	10	2	7	8	19	0	21	3	11	24	48
Totals	14	169	46	90	129	319	16	199	46	123	169	389

TABLE XXXV.

Per Cent.

1912–13	2.82	51.9	16.3	28.8	37.5		5.17	56.9	12.9	37.1	38.8
1913–14	3.19	59.5	10.6	27.6	36.2		3.06	51.0	16.3	33.7	40.8
1914–15	5.89	48.0	16.7	26.5	47.1		5.51	48.8	9.4	28.4	47.3
1915–16	5.26	52.6	10.5	36.8	42.1		3.33	43.8	6.2	22.9	50.0
Totals	4.39	53.0	14.4	28.2	40.4		4.11	51.1	11.8	31.6	43.5

TABLE XXXVI.

Deviation.

1912–13	+0.32	+1.9	+6.3	+3.8	−7.5	+2.67	+6.9	+2.9	+12.1	−6.2
1913–14	+0.69	+9.5	+0.6	+2.6	−8.8	+0.56	+1.0	+6.3	+8.7	−4.2
1914–15	+3.39	−2.0	+6.7	+1.5	+2.1	+3.01	−1.2	−0.6	+3.4	+2.3
1915–16	+2.76	+2.6	+0.5	+11.8	−2.9	+0.83	−6.2	−3.8	−2.1	+5.0
Totals	+1.89	+3.0	+4.4	+3.2	−4.6	+1.61	+1.1	+1.8	+6.6	−1.5

Since the positive deviations in the control experiments (when the card was not imaged) are about as large and are as consistent as in the regular experiments (when the card was imaged), we must infer that either the consistency of the deviations is fortuitous or the supposed influence for R cases is as freely operative in guesses upon unknown as upon "Imaged" cards. The latter alternative is supported by the consistency of the deviations only, since, with the possible exception of one under "Suit" (which, however, is matched by the sum of adjacent chance deviations in Sets *25* and *26* in Table XIV), no deviations exceed in size chance variation.[141]

Our data do not show by analyses, designed to detect an influence for R cases supposedly correlated with a feeling of certainty in guessing,

[141] *Cf.*, Table XVIII, *supra*, p. 66.

that such an influence was present. Tables of analyses, however, did furnish evidence which was only apparent; and consequently, besides vindicating the analyses themselves, they suggest the necessity for determining definitely how large a deviation in any particular case must be in order to be considered significant. This involves a statistical use of mathematical formulæ, which was referred to above in the discussion of methods of treating our results, and which will receive consideration immediately after the results of two more kinds of analysis, and a certain common criticism, are inspected.

Variation in Distance.

Upon the supposition that some influence for R cases concealed in our tables follows the law of the radiation of energy, we would expect the number of R cases to vary inversely with the square of the distance between experimenter and reagent. A segregation of results would then increase disproportionately the positive deviations from the probable per cent on the smaller distances. Only 847 experiments are available for this analysis, the deviations of which are shown below:

TABLE XXXVII.

Deviations, in Per Cent, of R Cases according to distance in Meters.

	Card Not Imaged						Card Imaged					
	R Cases				W	Tot.	R Cases				W	Tot.
Meters	Card	Color	No.	Suit			Card	Color	No.	Suit		
1.	+1.7	+3.7	−0.5	+1.3	−2.9	95	+1.2	−1.3	−0.3	−1.8	+2.5	82
2.	+1.5	+6.0	−4.0	+11.0	−3.0	50	−0.1	−0.1	+4.3	−2.6	−4.8	49
3.	+2.0	−9.1	+1.4	−5.7	+9.5	88	+1.4	−5.0	−0.8	+1.3	+6.3	76
4.6	+0.2	−4.1	+3.5	−0.7	+0.9	37	+0.5	−1.5	+8.2	−0.7	−2.6	33
6.	+0.4	+3.0	+0.8	+1.4	−2.8	102	+1.0	−2.1	+1.3	−2.4	+2.9	115
10.	+0.6	+7.8	−2.2	+3.1	−5.9	64	−2.5	−1.8	−4.6	−3.6	+1.4	56

Since the larger positive deviations are not so frequent in the "Card Imaged" part of the table as in the "Card not Imaged" part, are not so frequent for the smaller distances as for the greater, and are not conspicuously larger than the negative deviations, our data do not show that distance makes any difference to the supposed influence for R cases which consequently is not thereby detected.

Variation in Time.

If the time of the critical interval (while the experimenter is holding "imagery" of the card in his mind and the reagent is awaiting an

impression of a card or is making up his guess) is a function of R cases we might expect by tabulating R cases according to length of the interval to find a greater positive deviation in per cent of R cases on the optimal interval. In 24 sets the interval was varied from 10 to 60 seconds for the early series, which enables us to distribute 42 R cases on the "Card," when the "Card was Imaged," over the following intervals, in seconds.

TABLE XXXVIII.

Deviation in R Cases according to Time of Impression.

Interval (seconds)	*10*	*15*	*20*	*30*	*40*	*60*
R Cases	5	1	24	1	7	4
Number of experiments	220	70	1050	50	610	280
Per Cent R	2.3	1.4	2.3	2.0	1.1	1.4
Deviations	−0.2	−1.1	−0.2	−0.5	−1.4	−1.1

The absence of positive deviations indicates that there was no influence for R cases operative in these experiments which was a function of the time of the critical interval.[142]

Test for Retarded Effect.

Another supposition deserves notice here before we begin the mathematical treatment of our data. A criticism which is often made claims that our deviations may fail to show a telepathic influence because it is tardy in its effect; we might find it if we tabulated the coincidences between the card drawn and the *succeeding* instead of the contemporaneous guess.[143] Such a tabulation has been made from the data procured in 1913-14: 25 sets from the students and 10 sets from the "psychics." The following table of deviations, per 1000 experiments, is comparable with Table XVII, and includes the deviations for the total number of sets by students, as well as the deviations of the 2d succeeding guess for the psychics.

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[142] Apart from the variation in the length of the critical interval, the general method demanded from 15 to 20 seconds, which was used in all other sets. Of 21 Reagents for whom the interval was varied, 3 preferred 10 seconds, 13 preferred 20 seconds, and 5 preferred 40 seconds. In Lodge's experiments, in the Tyrol, in guessing of numbers on cards, the young ladies made as many as 10 or 12 guesses in a minute (Survival of Man, p. 64). The Rawson drawings were perceived by the percipient in 10 seconds (*Proceedings S. P. R.*, 12:9-11).

[143] Cases of deferred successes have been reported in the literature: *vide*, *Proceedings S. P. R.*, 1892, 8:548.

TABLE XXXIX.

Deviations of R Cases in Coincidences between card drawn—
And the First Succeeding Guess:

	Card Not Imaged.						Card Imaged.					
	R Cases				W	Tot.	R Cases				W	Tot.
	Card	Color	No.	Suit			Card	Color	No.	Suit		
Psychics	−0.7	−1.8	+0.8	−1.4	+1.5	467	−0.3	−3.5	−1.4	−1.6	+3.6	533
Students:												
Men, 4th 1000..	−1.2	−0.6	−0.9	−0.3	+1.7	482	+1.7	−1.6	+2.7	+1.2	0	518
Women, 5th 1000	+0.5	−5.4	+0.9	−1.6	+4.8	496	+0.3	+1.4	−1.3	−0.2	0	504
2500	−0.4	−2.7	+0.3	−0.6	+2.6	1229	+0.7	−0.6	+0.9	+0.4	+0.4	1271
And the Second Succeeding Guess:												
Psychics	+0.3	+1.8	−0.8	+4.1	−1.5	467	−0.6	-0.5	−2.5	+1.1	+1.3	533

The deviations under "Card Imaged" show no obvious advantage over those in the other part of the table, or over comparable deviations of R cases in coincidences between the card drawn and the preceding guess, and between cards drawn in the different sets, which have been found (see Table LIII, p. 129), nor is there significant augmentation of positive deviations when the R cases are distributed over the respective die-spots. There is no indication of an influence for R cases which is tardy in its effect.

Statistical Treatment of Data by Use of Mathematical Formulæ.

The other method of dealing with the data involves the application of mathematical formulæ to the data in the aggregate or to results of analysis, and leads to precision in the estimate of the significance of deviations. The "Card Not Imaged" side of Tables XIV and XVII furnishes chance deviations from the most probable number or the most probable per cent which have been of service in comparing the deviations in the regular experiments. But we have already found by analysis that causes beyond chance might yield deviations no larger (as in *C*, Set *V*, Table XIV),[144] and we have occasionally passed by, in our analyses, some positive deviations just a little larger than any chance deviations in the tables (*e. g.*, the +6.6 in the total under Suit, Table XXXVI). Evidently our reliance upon extreme chance variations is unsatisfactory, in a test for some influence beyond chance, especially when the influence may conceivably be slight and our deviations relatively small.

The mathematical theory of probability furnishes several tests by

[144] *Supra*, p. 63.

which such a slight influence might be detected. A distribution curve of the chance deviations follows a law from which an equal number of deviations resulting in part from other causes must depart. And formulæ, based upon this law, have been provided, by which the reliability of central measures or averages may be found. We can, then, (*a*) compare one group of deviations with another by means of distribution curves; and (*b*) we can apply probability formulæ to any particular measures in our tables.

Application of Probability Formulæ to Central Measures.

For the sake of illustrating the latter method, suppose we choose from Table XVII [145] a very favorable deviation, say +5.1 under "Suit" in the 4th 1000, and determine (1) the probability of its occurrence by chance alone, (2) how large it should be in order to be decisive as an indication of some cause in addition to chance, and (3) how many experiments should be performed in order to raise to scientific certainty the probability that it was not caused by chance alone.

(1) According to Bernoulli's Theorem,[146] if p and q are the probabilities of the occurrence and non-occurrence, respectively, of an event, it can be expected with a probability of

$$P = \frac{2}{\sqrt{\pi}} \int_0^{\gamma} e^{-t^2}\, dt + \frac{e^{-\gamma^2}}{\sqrt{2\pi pqn}},$$

that the actual results of observation (the per cent of observed occurrences) will lie within the field of

$$p \pm L,$$

where

$$L = \gamma \sqrt{\frac{2pq}{n}}.$$

[145] *Supra*, p. 65.

[146] *Vide*, E. W. Scripture: The New Psychology, 1910, p. 476. *Cf.*, also, F. Y. Edgeworth: The calculus of probabilities applied to Psychical Research. *Proceedings S. P. R.*, 1885, 3:190-9; 1886-87, 4:189-208.

Substituting, we have

$p = .25$ (Chance of guessing a suit)
$q = .75$ (Chance of missing a suit)
$n = 518$ (The number of experiments)
$L = .051$ (The deviation of R guesses from p)

and, by performing the operations indicated,[147]

[147]

$$\begin{aligned}
\log\ p &= 9.39794 - 10\\
\log\ q &= 9.87506 - 10\\
\log\ 2 &= 0.30103\\
\hline
\log\ 2pq &= 19.57403 - 20\\
\log\ n &= 2.71433\\
\hline
\operatorname{colog} \frac{2pq}{n} &= 3.14030\\
\hline
\operatorname{colog} \sqrt{\frac{2pq}{n}} &= 1.57015 = \log \frac{\gamma}{L}\\
\log\ L &= 8.70757 - 10\\
\hline
& 0.277720 = \log \gamma\\
\gamma &= 1.8955
\end{aligned}$$

$$\begin{aligned}
\log\ \gamma^2 &= 0.55544\\
\log \log\ e &= 9.63778 - 10\\
& 0.19322 = \log \log e^{\gamma^2}
\end{aligned}$$

$$\begin{aligned}
\log e^{\gamma^2} &= 1.56040\\
\log\ n &= 2.71433\\
\log \sqrt{\pi} &= 0.24857\\
\hline
& 4.52330\\
\log \frac{\gamma}{L} &= 1.57015\\
\hline
& 7.04685 - 10 = \log \Psi
\end{aligned}$$

$$\begin{aligned}
\Psi &= 0.001114\\
\Phi(\gamma) &= 0.9927\\
\hline
P &= 0.993814
\end{aligned}$$

The $\Phi(\gamma)$ value was found directly from Bruno Kämpfe's "Tafel des Integrals $\Phi(\gamma) = \frac{2}{\sqrt{\pi}} \int_0^{\gamma} e^{-t^2}\ dt$" (Wundt's *Philosophische Studien*, 1894, Bd. 9:150) arranged for values of γ.

we find $$\gamma = 1.8955,$$

and Ψ, the last term in the P-equation above, and $\Phi(\gamma)$, the first term:

$$\Psi = .001114$$
$$\Phi(\gamma) = .9927$$
$$P = .993814;$$

whence we get 0.9938 for the probability that chance deviations will not exceed this limit of 5.1%; or, to state the conclusion in another way, we may expect in 10,000 sets of 518 guesses on suit, 62 chance deviations greater than 5.1%. Since this value, then, lies within the field of chance deviation, although the probability of its occurrence by chance is fairly low, it cannot be accepted as a decisive indication of some cause beyond chance which operated in favor of success in guessing.

(2) Let us determine how large the deviation should have been in order to indicate with scientific certainty that some cause in addition to chance was operative.

To make an application of Bayes's Theorem, known as the Theorem of Poisson,[148] it may be determined with a probability of

$$P = \frac{2}{\sqrt{\pi}} \int_0^{\gamma} e^{-t^2}\, dt$$

that deviations resulting from chance causes alone will lie within the limit of

$$p \pm L$$

when

$$L = \gamma \sqrt{\frac{2pq}{n}};$$

[148] *Vide,* Wahrscheinlichkeitsrechnung, in Encyklopädie der mathematischen Wissenschaften mit Einschluss ihrer Anwendungen, Bd. 1, Teil II, S. 762; also Scripture: Some psychological illustrations of the theorems of Bernoulli and Poisson, *Am. Jr. Psychology,* 1893-94, 6:431-432; and Todhunter: A History of the Mathematical Theory of Probability. Cambridge, 1865, pp. 552-7.

It will be noticed that this formula differs from James Bernoulli's, which was used above, in dispensing with the second term (Ψ) in the P equation. Since we here assign to γ the value of 3, the value of Φ (the first term) becomes .9999779 (*cf.,* Todhunter, *op. cit.,* p. 553; or, Bertrand: Calcul des Probabilités, Paris, 1889, p. 331), and the value of Ψ, the omitted term, becomes negligible. For

and, if we meet the requirement of a degree of accuracy usual in scientific work by making

$$P = 0.9999779,$$

when absolute certainty is

$$P = 1,$$

then, in the above formulæ,

$$\gamma = 3.$$

Substituting, and performing the operations, we get

$p = .25$ $\quad \log p = 9.39794 - 10$
$q = .75$ $\quad \log q = 9.87506 - 10$
$n = 518$ $\quad \operatorname{colog} n = 7.28567 - 10$
$\gamma = 3$ $\quad \log 2 = 0.30103$

$$26.85970 - 30$$
$$13.42985 - 15 = \log \sqrt{\frac{2pq}{n}}$$
$$\log \gamma = 0.47712$$
$$13.90697 - 15 = \log L$$
$$L = 0.08072.$$

The deviation, therefore, should have been as great as 0.0807, or 8.1%, in order to be satisfactory evidence for some cause in addition to chance.

(3) Supposing that the deviation of +5.1% could be maintained in continuing the experiments, let us determine how many experiments

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example, with this assigned value of γ, and with all the other values in the preceding problem remaining unchanged, the value of Ψ becomes .000004996, instead of .001114, which was found when $\gamma = 1.8955$. The value of Ψ varies inversely with $\sqrt{n}$ and with $\sqrt{pq}$, and both of these factors are given various values in our respective problems. It remains, therefore, to show the limits, in each of these cases, within which the Ψ value varies, in order to demonstrate that it is negligible:

If $pq = .25$, and $n = 518$, $\Psi = .000004996$
$n = 58$, $\Psi = .000014998$
$n = 4662$, $\Psi = .000001665$

If $n = 518$, and $p = .25$, $pq = .1875$, $\Psi = .000004996$
$p = .5$, $pq = .25$, $\Psi = .0000043315$
$p = .025$, $pq = .024375$, $\Psi = .000013872$

If $n = 58$, and $p = .025$, $pq = .024375$, $\Psi = .000041616$

This last value of Ψ is the largest it can have in our use of the formula, and it is limited to the cases on the Whole Card ($p = .025$) in sets of 50.

The effect of dropping Ψ, then, is that, for whatever value we are determining, the probability is slightly greater than the value of Φ, (.9999779).

should constitute the set in order to increase the probability to scientific certainty that the deviation was not caused by chance alone.

By reversing Bayes's Theorem,[149] we can determine with a certainty of

$$P = \frac{2}{\sqrt{\pi}} \int_0^{\gamma} e^{-t^2}\, dt$$

that the number of experiments must be

$$n = \frac{2\gamma^2 pq}{\delta^2};$$

and, if we demand that

$$P = 0.9999779,$$

then

$$\gamma = 3$$

and

$$n = \frac{18pq}{\delta^2}.$$

Substituting, and performing the operations, we get

$p = .25$	colog δ =	1.29243
$q = .75$	colog δ^2 =	2.58486
$\delta^2 = .051$	log p =	9.39794 — 10
	log q =	9.87506 — 10
	log 18 =	1.25527
	log n =	23.11313 — 20

$$n = 1297.6.$$

If, therefore, the deviation of +5.1% were maintained in the guessing of suit, and the number of experiments were increased from 518 to 1298, the evidence for a cause in addition to chance, operative for success in guessing, would be satisfactory.

In like manner the largest positive deviations in the tables of results might be inspected and their reliabilities as evidence for an extra-chance cause determined. It will suffice, however, if (*a*) the largest deviations in the "Card Imaged" part of Table XVII [150] are portrayed in tabular view with the limit of chance deviation which they should equal or ex-

[149] *Vide*, Scripture: *Am. Jr. Psychology*, 1893-94, 6:431-2.

[150] *Supra*, p. 65.

ceed in order to be trustworthy indications of an extra-chance cause; and (*b*) reference tables and corresponding charts are provided, which give the limits of chance deviation for sets varying in number of experiments, in order to facilitate an estimate of the value of any deviation in the tables.

In both cases we assume a scientific standard of certainty by putting

$$P = 0.9999779,$$

when absolute certainty is

$$P = 1;$$

and we use again the formula for the limit of chance deviation which we have employed in the illustrations above:[151]

$$L = \gamma \sqrt{\frac{2pq}{n}},$$

in which, for the stated standard of certainty,

$$\gamma = 3.$$

[151] Although there are certain restrictions to the application of this formula, such as when n (the number of experiments) is small, or when p is very small, its use is peculiarly applicable to the type of data with which we are dealing, and it is not unsupported by approved statistical methods already used in the field of psychical research. *E. g.*, in his "Analysis of Mrs. Verrall's Card Experiments" (*Proceedings S. P. R.*, 1895, 11 : 193-7) Mr. C. P. Sanger used the following formula for the limit of chance deviation from the *probable number* of occurrences:

$$K = 3\sqrt{2(1-\varrho)m\varrho},$$

in which

ϱ = the probability of occurrences,
m = the number of experiments;

and it reduces to our formula for the limit of chance deviation from the *probability* of occurrence:

$$L = 3\sqrt{\frac{2pq}{n}},$$

in which

p = probability of occurrence,
n = number of experiments,

as follows:

$$K = 3\sqrt{2(1-\varrho)m\varrho};$$

changing notation,

$$\varrho = p,$$
$$1 - \varrho = q,$$
$$m = n;$$

The following table displays (*a*) the probable per cent of R cases, (*b*) the largest per cent of R cases made in the "Card Imaged" experiments,[152] (*c*) the largest deviations from probability,[153] (*d*) the calculated limit of chance deviation,[154] (*e*) the ratio of deviation to limit $\left(\frac{x}{L}\right)$,

rewriting,

$$K = 3\sqrt{2pqn};$$

and reducing to the per cent form,

$$\frac{K}{n} = \frac{3\sqrt{2pqn}}{n},$$

$$\frac{K}{n} = L = 3\sqrt{\frac{2pqn}{n^2}},$$

$$= 3\sqrt{\frac{2pq}{n}}.$$

Its relation to other customary formulæ may be seen by considering Yule's "practical" limit in terms of the "Standard Error" (An Introduction to the Theory of Statistics, 3d ed., pp. 266-7):

$$3\sigma = 3\sqrt{\frac{pq}{n}}.$$

Our limit is definitely greater:

$$L = \sqrt{2} \times 3\sigma.$$

But of his limit Yule says: "We know roughly that the great bulk at least of the fluctuations of sampling lie within a range of three times the standard-deviation; and if an observed difference from a theoretical result greatly exceeds these limits it cannot be ascribed to a fluctuation of simple sampling: it may therefore be significant" (p. 266); and in applying it to the results of 49,152 throws of a die, he says, "The deviation observed is 5.1 times the standard error, and, practically speaking, could not occur as a fluctuation of simple sampling. It may perhaps indicate a slight bias in the dice" (p. 267). And if a table of values of the Probability Integral (as in Davenport: Statistical Methods, 3d ed., pp. 119-125) is consulted, it will be found that for the limit of $+3\sigma$

$$P = 0.99865;$$

which permits about 135 cases of chance deviation beyond the limit in 100,000 sets, while our limit (L) with a probability of

$$P = 0.9999779,$$

permits but 2 cases.

[152] From Table XVI, *supra*, p. 64.
[153] From Table XVII, *supra*, p. 65.
[154] *Cf.*, Table XLI, *infra*, p. 89.

(*f*) the ratio of deviation to the "Standard Error" $\left(\frac{x}{\sigma}\right)$[155] and (*g*) the calculated probability of the given deviation as the limit of chance deviation (*P*):[156]

TABLE XL.

The Highest Values in the Results of Card-Guessing Compared with Standard Theoretical Values.

	Card	Color	No.	Suit
(*a*) Probable per cent of R cases.....	2.5	50.0	10.0	25.0
(*b*) Largest per cent of R cases.....	3.86	54.8	13.1	30.1
(*c*) Largest deviations (*x*).....	1.36	4.8	3.1	5.1
(*d*) $L=3\sqrt{\frac{2pq}{n}}$	2.96	9.5	5.7	8.2
(*e*) $\frac{x}{L}$	0.460	0.505	0.544	0.622
(*f*) $\frac{x}{\sigma}$	1.950	2.141	2.307	2.637
(*g*) *P*	0.9474	0.9839	0.9895	0.9959

A glance at the $\frac{x}{L}$ values shows that the largest deviations in our sets of about 500 guesses on cards, when the card was held in the mind of the experimenter, range from but 46% to 62.2% of the limit of

[155] The ratio $\frac{x}{\sigma}$ is derived from $\frac{x}{L}$:

$$L=3\sqrt{\frac{2pq}{n}},$$

$$\sigma=\frac{L}{\sqrt{2}\times 3},$$

$$\frac{x}{\sigma}=\frac{x}{\frac{L}{\sqrt{2}\times 3}}=\sqrt{2}\times 3\frac{x}{L}=4.24\frac{x}{L}.$$

[156] The probability of the deviation is taken from Davenport's Table of Values of the Probability Integral Corresponding to Values of x/σ (*op. cit.*, pp. 119 ff.), which assumes a "normal" distribution (*i. e.*, when $p=q$), and, consequently, is subject to a slight error when applied to the values in the table, other than under Color, which tends to raise the probability; for example, by Bernoulli's Theorem we found (*supra*, p. 82) for the deviation 5.1, under Suit,

$$P=0.9938,$$

while in the table it is

$$P=0.9959,$$

a difference of

$$+0.0021.$$

chance deviation; and the P values also show that from 4 to 53 out of every 1000 chance deviations may be expected to exceed our highest empirical deviations, when sets of 500 guesses are considered.

The other deviations, in the sets of 50, and in the totals of 5000, found in Tables XIV and XVII,[157] may be compared with the following tables and charts[158] for their evaluation.

Table XLI and Plate III portray in per cent the limits of chance deviation, hence, the minimal significant deviations from the most probable per cent of R cases, for sets of 50 to 10,000 experiments; and Table XLII[159] and Plate IV show the same facts, with smaller gradations in number of experiments, for sets of 50 to 1000.

Tables XLI*a* and XLII*a* give the corresponding lowest significant per cents of R cases for sets of the same sizes.

The curves are smoothed and the least probable and most probable *per cents of R cases,* due to chance alone, may be read or estimated, for a set of any number of experiments, between the extremes listed in the tables, from the figures on the *left margin. The number of experiments* in the set is shown at the *bottom* of the plate. On Plate IV, for example, (1) if one wishes to learn whether 16% R cases in a set of 250 guesses on number is significant of thought-transference, the point coördinate with *16* on the margin and *250* at the bottom may be located, and found to lie within the field of chance deviation; (2) if he wishes to know how large it should be to be significant, the point in the limiting curve above *250* will be found to be coördinate with the desired value *in per cent* on the left margin—about 18%; and (3) if he wishes to know how many experiments to perform in order that 16% R cases may be decisive, the 16% ordinate may be followed to its point of intersection with the limiting curve, and at the bottom of the plate the required number of experiments may be read off or estimated—about 480. The base-line for each characteristic of the card shows the probability

[157] *Supra,* pp. 57-63, 65.

[158] Several purposes besides our immediate interest are served by these tables and charts: They may be found of value by others who desire to engage in further investigation of thought-transference; and they illustrate very clearly the adequacy of scientific procedure to search out and identify even a slight cause in addition to chance by carrying out the experiments to a sufficiently great number. The layman will not feel so hopeless in the face of slight deviations, when he has reason to believe them significant, if he can see from this illustration that the limit of chance deviation is lowered directly with the square root of the number of experiments in carrying further the investigation.

[159] *Infra,* p. 90.

of a single guess upon that characteristic (*e. g., 10%* for Number, *25%* for Suit); which is also the most probable per cent for any number of guesses.

It is doubtless evident that the relation of any deviation in Tables XIV [160] and XVII to its chance-limit may be found by locating the size of the set at the bottom of the plate, counting upward from the proper base-line, and noting the position of the point found with reference to the accompanying limiting curve. If it lies above the limiting curve it is significant.

TABLE XLI.

Minimal Significant Deviations.

(In Per Cent) $L = \pm\gamma\sqrt{\frac{2pq}{n}}$

Number of experiments in set.	Card	Color	Number	Suit
Probable %..	**2.50**	**50.0**	**10.0**	**25.0**
50*.....	9.37	30.0	18.0	26.0
100.....	6.63	21.2	12.7	18.4
500†.....	2.96	9.5	5.7	8.2
1000.....	2.09	6.7	4.0	5.8
1600‡.....	1.66	5.3	3.2	4.4
2000.....	1.48	4.7	2.8	4.1
3000.....	1.21	3.9	2.3	3.3
5000§.....	0.94	3.0	1.8	2.6
10000.....	0.66	2.1	1.3	1.8

*Deviations comparable with those in Table XIV after the latter are multiplied by 2; or the former are divided by 2 (As, 4.96 15.0 9.0 13.0).

†Comparable with Table XVII, without reduction.

‡Comparable with Table XXV.

§Comparable with "Total" in Table XVII.

[160] The deviations taken from Table XIV must be reduced to per cent, however, before they may be compared with the values in these tables and plates; this may be done by multiplying them by 2.

TABLE XLI*a*.

Minimal Per Cent of R Cases to be Significant.

Number of experiments in set.	Card	Color	Number	Suit
Most Probable Per Cent..	**2.50**	**50.0**	**10.0**	**25.0**
50	11.87	80.0	28.0	51.0
100	9.13	71.2	22.7	43.4
500	5.46	59.5	15.7	33.2
1000	4.59	56.7	14.0	30.8
1600	4.16	55.3	13.2	29.4
2000	3.98	54.7	12.8	29.1
3000	3.71	53.9	12.3	28.3
5000	3.44	53.0	11.8	27.6
10000	3.16	52.1	11.3	26.8

TABLE XLII.

Minimal Significant Deviations in Per Cent, for Sets of 50-1000.

50	9.37	30.0	18.0	26.0
100	6.63	21.2	12.7	18.4
150	5.52	17.3	10.4	15.0
200	4.68	15.0	9.0	13.0
300	3.83	12.2	7.4	10.6
400	3.31	10.6	6.4	9.2
500	2.96	9.5	5.7	8.2
600	2.71	8.7	5.2	7.5
700	2.50	8.0	4.8	7.1
800	2.34	7.5	4.5	6.5
900	2.21	7.1	4.2	6.1
1000	2.09	6.7	4.0	5.8

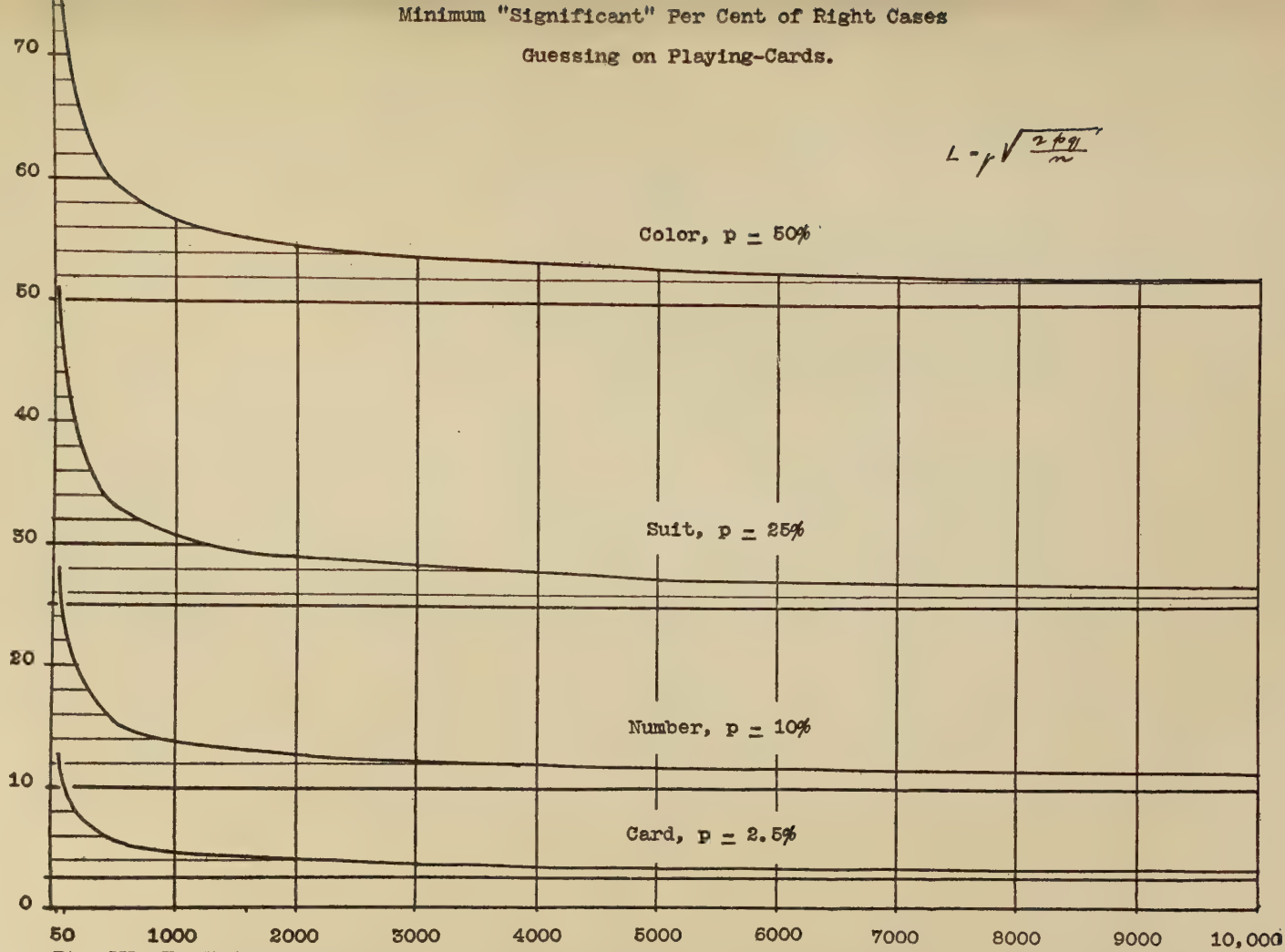

Plate III.—The limits of chance deviation in R cases on Card, Color, Number, and Suit, for sets of 50 to 10,000 experiments. The ordinates give the per cent of R cases; the abscissæ give the number of experiments in a set; the heavy ordinate shows the most probable per cent of R cases; the curve shows the highest per cent of R cases that can be attributed to chance causes. From Table XLI*a*.

TABLE XLII*a*.

Minimal Per Cent of R Cases to be Significant. In Sets of 50-1000.

Number of experiments in set.	Most Probable Per Cent..	Card	Color	Number	Suit
		2.50	**50.0**	**10.0**	**25.0**
50		11.87	80.0	28.0	51.0
100		9.13	71.2	22.7	43.4
150		8.02	67.3	20.4	40.0
200		7.18	65.0	19.0	38.0
300		6.33	62.2	17.4	35.6
400		5.81	60.6	16.4	34.2
500		5.46	59.5	15.7	33.2
600		5.21	58.7	15.2	32.5
700		5.00	58.0	14.8	32.1
800		4.84	57.5	14.5	31.5
900		4.71	57.1	14.2	31.1
1000		4.59	56.7	14.0	30.8

By reference to the tables above the reader may verify the following survey of the highest deviations to be found in our data.

The largest deviations in Table XIV (shown in Table XVIII)[161] compared with the largest chance deviations as calculated from the formula and tabulated for sets of 50 in Table XLI:

	Card Not Imaged.				Card Imaged			
	Card	Color	No.	Suit	Card	Color	No.	Suit
Probable Number....	**1.25**	**25**	**5**	**12.5**				
Table XIV.....	+5	+13	+5	+8	+5	+7	+6	+8
$L = \gamma\sqrt{\frac{2pq}{n}}$.....	+4.69	+14.9	+9.0	+13.0				

The largest deviations in Table XVII[162] in comparison with calculated chance deviation, per set of 500:

	Card Not Imaged.				Card Imaged.			
	Card	Color	No.	Suit	Card	Color	No.	Suit
Probable Per Cent...	**2.50**	**50**	**10**	**25**				
Table XVII.....	+2.23	+4.8	+2.7	+3.8	+1.36	+4.8	+3.1	+5.1
$L = \gamma\sqrt{\frac{2pq}{n}}$.....	+2.96	+9.5	+5.7	+8.2				

The deviations of the total in Table XVII[162] in comparison with calculated chance deviation, per set of 5000;

[161] *Supra*, p. 66.
[162] *Supra*, p. 65.

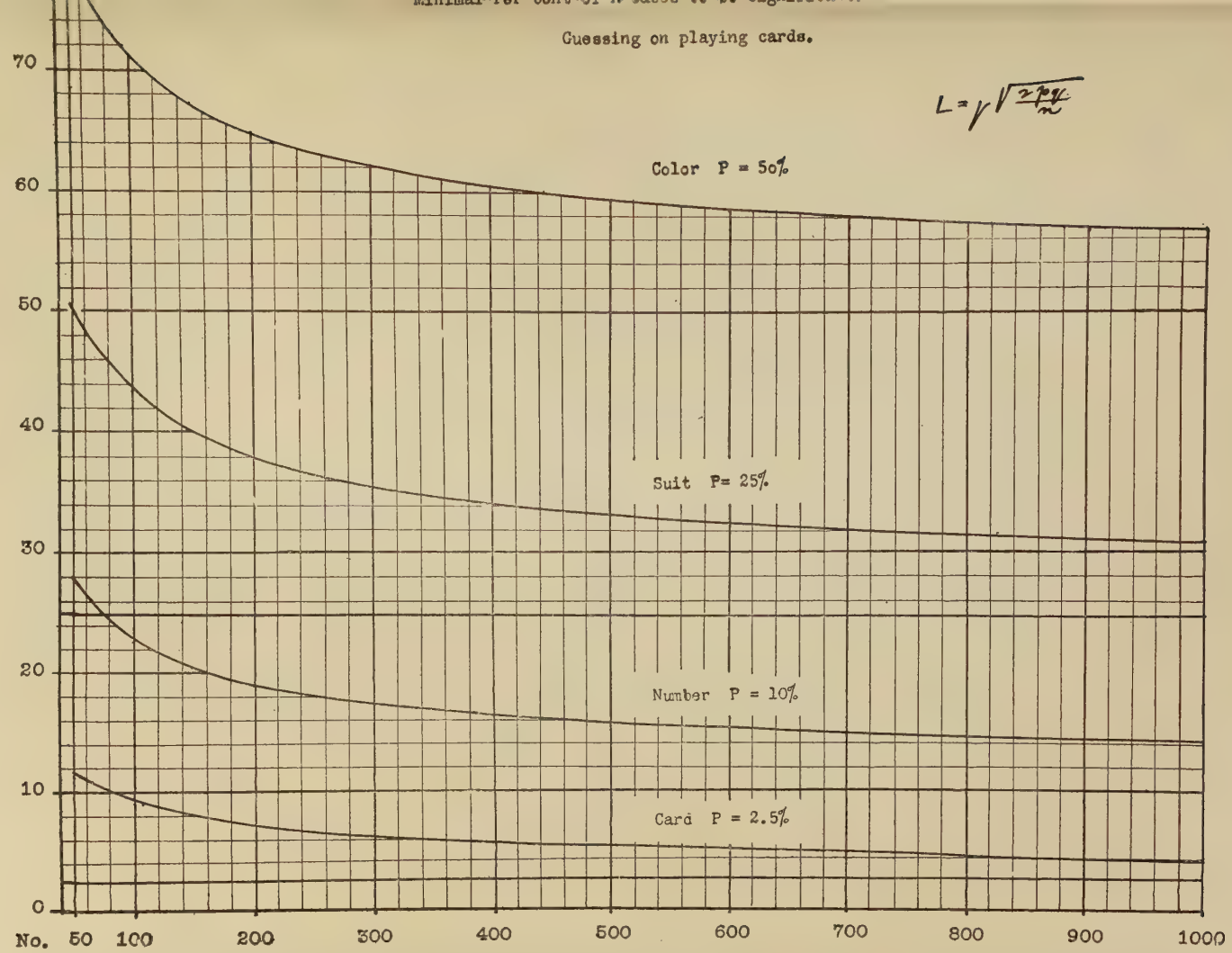

Plate IV.—The limits of chance deviation in R cases on Card, Color, Number, and Suit, for sets of 50 to 1000 experiments. The ordinates give the per cent of R cases; the abscissæ give the number of experiments in a set; the heavy ordinate shows the most probable per cent of R cases; the curve shows the highest per cent of R cases that can be attributed to chance causes. From Table XLII*a*.

	Card Not Imaged.				Card Imaged.			
	Card	Color	No.	Suit	Card	Color	No.	Suit
Probable Per Cent....	**2.50**	**50**	**10**	**25**				
Table XVII.....	+0.40	+1.2	0.0	+0.8	+0.48	−0.3	+0.5	+1.2
$L = \gamma\sqrt{\frac{2pq}{n}}$.....	+0.94	+3.0	+1.8	+2.6				

It is pretty evident that, taking our data in the mass and selecting the largest deviations, although the calculated limit of chance deviation is sometimes approached, it is never (with the exception of the +5 under Card in Table XIV) equaled; and the larger the set considered, the more does it exceed the deviations in the data. This means, then, that, whether the largest deviations in the single sets, or the deviations in the totals of 10 sets or of 100 sets, are considered, no cause besides chance is revealed in sufficient magnitude to meet the usual statistical requirements for identification.

That a real cause may be present and yet be veiled in the chance variation has already been considered in the discussion of the use of analyses for magnifying it sufficiently for detection; but this consideration is worthy another glance here since in the last five sets (*I* to *V*) in Table XIV, *C*,[163] we find, on the "Card Imaged" side of the table, a number of deviations from the probable number for a set of 50, which we know[164] are the result in part of other causes besides chance, and which fall within the limits of chance deviation as calculated from our formula; under "Color" +13, +12, and +7 fall within ±14.9, and under "Suit" +11 and +3 fall within ±13. One cannot be in doubt about the existence of an extra-chance cause in the whole group, because in the averages the deviations of the per cent of R cases from the probable per cent exceed in considerable amount the calculated limit of chance deviation:

	Card Not Imaged.					Card Imaged.				
	Card	Color	No.	Suit	Total	Card	Color	No.	Suit	Total
Probable %.....	**2.50**	**50**	**10**	**25**						
Sets *I–V*.....	+0.36	−1.8	+3.0	+1.1	245	+41.40	+29.2	+49.6	+29.5	255
$L = \gamma\sqrt{\frac{2pq}{n}}$....	+4.19	+13.4	+8.0	+11.6						

And the deviations are magnified by analysis:—Distribution over the die-spots:

	Die-Spot Even						Die-Spot Odd					
Die-Spot	R Cases Card	Color	No.	Suit	W	Number	Die-Spot	Card	Color	No.	Suit	W	Number
2	+1.45	−7.9	+5.8	+4.0	+3.7	76	*1*	+60.90	+47.1	+77.2	+44.4	−43.0	101
4	+1.40	+7.1	+3.0	+1.0	−7.4	77	*3*	+3.30	−2.3	+2.8	−2.9	+3.8	86
6	−0.41	−4.4	+0.9	−0.1	+1.7	92	*5*	+60.70	+42.7	+68.0	+48.5	−39.1	68

[163] *Supra*, p. 63.

[164] *Vide, infra*, pp. 121 ff.

The hope of bringing to light some influence working for R cases, by the use of calculated limits of chance deviation, then, lies in extending the experiments to large numbers. If individual deviations which result in part from causes besides chance lie in a set of experiments too small in number to raise them above the calculated limit of chance variation, their significance must be determined by some other method. If they escape the method of analysis, illustrated some pages above, there remain the method of distribution, and the method of comparing the single measures of the regular (Card Imaged) experiments with those of the control (Card Not Imaged) experiments.

Comparison of the Empirical with their Theoretical Distributions.

According to the theory of probability, when the events the distribution of which is sought are "discontinuous," [165] such as ours, the frequencies of the successive variates (numbers of R cases in an aggregate of sets) are given by the successive terms of the expansion of the binomial theorem,[166]

$$N(q+p)^n$$

when

$N =$ the number of sets of experiments
$n =$ the number of experiments in a set
$p =$ the probability of a single occurrence
$q = 1 - p$.

As, in the case of 64 guesses on color, when the R guesses are counted in each of 16 sets of 4 experiments:

$$16\,(q+p)^4 =$$
$$16\,(q^4 + 4q^3p + 6q^2p^2 + 4qp^3 + p^4);$$

but in this case the probability of a single R guess is $\frac{1}{2}$, hence

$$p = q = \tfrac{1}{2},$$

[165] That is, are exact quantities, like the number of Right guesses on Color in a set of 50, as distinguished from "Continuous" events, which are approximated quantities, like the height of a man read from a scale.

[166] Yule (*op. cit.*, p. 293) gives the following rule: "The frequencies 0, 1, 2, . . . successes in N trials of n events are given by the successive terms in the binomial expansion of $N(q+p)^n$, *viz*:

$$N\left\{q^n + n\cdot q^{n-1}p + \frac{n(n-1)}{1\cdot 2}\,q^{n-2}p^2 + \frac{n(n-1)(n-2)}{1\cdot 2\cdot 3}\,q^{n-3}p^3 + \ldots\right\}".$$

and the equation becomes

$$16\,[(\tfrac{1}{2})^4 + 4(\tfrac{1}{2})^4 + 6(\tfrac{1}{2})^4 + 4(\tfrac{1}{2})^4 + (\tfrac{1}{2})^4] = 1 + 4 + 6 + 4 + 1.$$

Consequently we have:

Frequency of R guesses ..... =	1	4	6	4	1
Variates (or number of R guesses in the set) =	*0*	*1*	*2*	*3*	*4*

Thus, one set would have no R guesses, four sets would have 1 R guess, 6 sets 2 R guesses, four sets 3 R guesses, and one set 4 R guesses. This is the theoretical distribution,[167] and from it one could draw the curve of distribution.

If some cause in addition to chance operated for R guesses, the empirical distribution would be expected to depart, in extent corresponding to the efficiency of the cause, from the theoretical distribution, and it might be detected by a comparison of the distributions; the empirical curve would be shifted in the direction of the higher variates.

It may be noted that in the survey of our largest positive deviations in relation to the limit of chance deviations, which we have just concluded, we compared only the single values at the upper limit of the empirical and the theoretical distributions. We shall later compare the mean values. Now we are engaged in comparing all of the values in the empirical distributions with their respective theoretically expected magnitudes; and not each value singly, but the proportion in which all of the values in any aggregate of sets are distributed.

The following tables and curves offer the data for visual inspection: the degree to which an empirical distribution is shifted above or below the theoretical distribution can be determined to be significant only by the application of formulæ (*e. g.*, Pearson's formulæ for "Closeness of

[167] When n is large it is found convenient to use the well-known formula given by H. Laurent (*Calcul des Probabilités*) in computing the theoretical distribution: It is a formula for the probability of k occurrences, in a set of n independent trials, of an event of which the probability of a single occurrence in one trial is equal to λ:

$$\frac{n!}{k!\,(n-k)!}\lambda^k\,(1-\lambda)^{n-k}$$

in which $n!$ is "n factorial":

$$n! = n(n-1)(n-2)\ \ldots\ 3\cdot 2\cdot 1$$

This formula was used, unless otherwise stated, for computing each of the theoretical distributions in the following tables. For factorials of large numbers we used the approximate exponential formula given by Laurent (p. 13):

$$n! = n^n e^{-n}\sqrt{2\pi n}$$

"Fit," and its probability), and this determination will be deferred[168] until formulæ based upon empirical deviations are applied to single measures, in the next section.

In the following tables x is the number of R judgments in a set of 50; C stands for Control experiments, "Card Not Imaged"; R, for Regular experiments, "Card Imaged"; P, for the theoretically Probable number. The Residuals are the deviations from P. Under C, R, and P, are entered the number of sets. The whole number of sets upon which all the tables are based is 100.

Table XLIII gives the distribution of R judgments on the whole "Card"; Table XLIV, on "Color"; Table XLV, on "Number"; Table XLVI, on "Suit"; and Table XLVII, guesses wholly wrong; and the accompanying Plates V-IX illustrate the facts in the "Exactly x" column.

TABLE XLIII.

Distribution of R Cases on the "Card" in 100 sets of 50.
Probability 1 : 40 (2.5%). Normal Reagents.

x	Occurrence of Numbers						Residuals				Difference R–C	
	C	R	P	C	R	P	C	R	C	R	Below	Exactly
	Below x			Exactly x			Below x		Exactly x		x	x
0	0	0	0	24	20	28	0	0	−4	−8	0	−4
1	24	20	28	32	36	36	−4	−8	−4	0	−4	+4
2	56	56	64	30	27	23	−8	−8	+7	+4	0	−3
3	86	83	87	8	8	9	−1	−4	−1	−1	−3	0
4	94	91	96	4	7	3	−2	−5	+1	+4	−3	+3
5	98	98	99	1	1	1	−1	−1	0	0	0	0
6	99	99	100	1	1	0	−1	−1	+1	+1	0	0
7	100	100	100	0	0	0	0	0	0	0	0	0

From Table XLIII we note that less than one R case on the whole card occurred in 24% of the "Control" (Card Not Imaged) sets, 20% of the "Regular" (Card Imaged) sets, while 28% is recorded as the theoretical expectation, giving residuals of −4 and −8 respectively; and that Exactly one R case occurred in 32% of the "Control" sets and 36% of the "Regular" sets, with a probability of 36%, giving residuals of −4 and 0 respectively; the Difference R–C was −4 and +4 respectively.

The "Residuals below x" show by their consistent negative signs that both empirical curves are somewhat shifted toward the higher vari-

[168] *Vide, infra,* p. 108.

ates, and the corresponding *R–C* differences "Below *x*" show that some slight advantage is held by the regular distribution. The "Residuals Exactly *x*" show that the advantage was made on the variates *2* and *4*, and the "Difference *R–C* Exactly *x*" shows the Regular distribution to hold the advantage on variate *4*. The accompanying curves (Plate V) give a pictorial view of the relations existing between the three distributions. Whether the advantage of the Regular distribution is significant will be shown in the next section.

TABLE XLIV.

Distribution of R Cases on "Color" in 100 sets of 50.
Probability 1 : 2 (50%). Normal Reagents.

x	Occurrence of Numbers						Residuals				Difference *R–C*	
	Below *x*			Exactly *x*			Below *x*		Exactly *x*		Below	Exactly
	C	*R*	*P**	*C*	*R*	*P*	*C*	*R*	*C*	*R*	*x*	*x*
16	0	0	0	1	0	0	0	0	+1	0	0	−1
17	1	0	1	0	1	1	0	−1	−1	0	−1	+1
18	1	1	2	3	4	2	−1	−1	+1	+2	0	+1
19	4	5	3	2	4	3	+1	+2	−1	+1	+1	+2
20	6	9	6	9	5	4	0	+3	+5	+1	+3	−4
21	15	14	10	1	8	6	+5	+4	−5	+2	−1	+7
22	16	22	16	4	2	8	0	+6	−4	−6	+6	−2
23	20	24	24	8	8	10	−4	0	−2	−2	+4	0
24	28	32	34	9	8	11	−6	−2	−2	−3	+4	−1
25	37	40	44	13	12	11	−7	−4	+2	+1	+3	−1
26	50	52	56	11	19	11	−6	−4	0	+8	+2	+8
27	61	71	66	10	8	10	−5	+5	0	−2	+10	−2
28	71	79	76	7	4	8	−5	+3	−1	−4	+8	−3
29	78	83	84	6	11	6	−6	−1	0	+5	+5	+5
30	84	94	90	6	2	4	−6	+4	+2	−2	+10	−4
31	90	96	94	3	1	3	−4	+2	0	−2	+6	−2
32	93	97	97	3	3	2	−4	0	+1	+1	+4	0
33	96	100	98	1	0	1	−2	+2	0	−1	+4	−1
34	97	100	99	0	0	0	−2	+1	0	0	+3	0
35	97	100	100	1	0	0	−3	0	+1	0	+3	−1
36	98	100	100	1	0	0	−2	0	+1	0	+2	−1
37	99	100	100	0	0	0	−1	0	0	0	+1	0
38	99	100	100	1	0	0	−1	0	+1	0	+1	−1
39	100	100	100	0	0	0	0	0	0	0	0	0

*The apparent discrepancies in this column are accountable for by the summation of decimals which were negligible in the *P* column under "Exactly *x*." The nearest integral number was chosen in both columns.

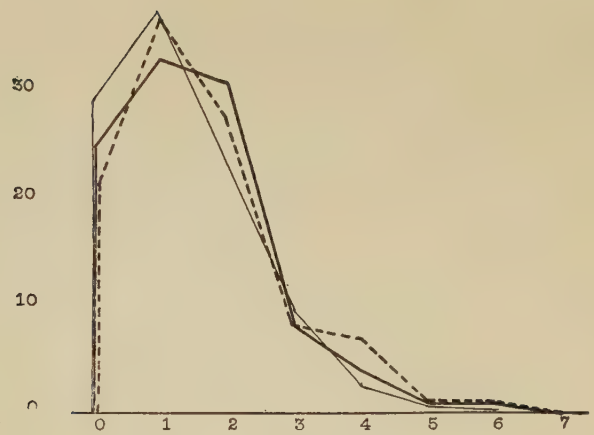

Plate V.— R Guesses on the "Card."

Each curve includes 100 sets, of 50. Probable number, 1.25.
(Solid line, "Card Not Imaged"; broken line, "Card Imaged"; light line, Probability.)

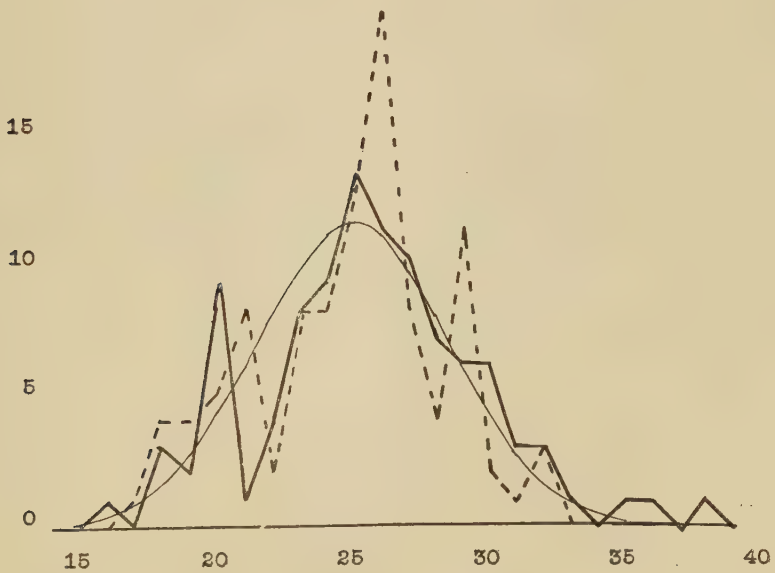

Plate VI.— R Guesses on the "Color."

100 sets, of 50, in each curve. Probable number, 25.
(Solid line, "Card Not Imaged"; broken line, "Card Imaged"; light line, Probability.)

From Table XLIV we note that there were less than *24* (x) R cases on "Color" in 28% of the "Card Not Imaged" sets (C), 32% of the "Card Imaged" sets (R), and 34% in the chance distribution (P), giving residuals of –6 and –2; and that there were Exactly *24* R cases in 9% of the "Card Not Imaged" sets, and in 8% of the "Card Imaged" sets, while it is expected in 11% of sets of chance events, giving residuals of –2 and –3 respectively; that the Difference R–C for "Below x" is +4 and for "Exactly x" is –1. The table shows both sets of experiments to approximate fairly closely the theoretical values. The facts in the "Exactly x" column are more clearly shown in the curves of Plate VI. That the larger residuals are not so distributed as to be significant may be better appreciated if the data of the table under "Occurrence of numbers, Exactly x" are grouped in fives about the central variate (*25*), and the broader relations between R and C and P are displayed:

x	C	R	P
Below *18*	1	1	1
18–22	19	23	23
23–27	51	55	53
28–32	25	21	23
33–37	3	0	1
Above *37*	1	0	0

TABLE XLV.

Distribution of R Cases on "Number" in 100 sets of 50.
Probability 1:10 (10%). Normal Reagents.

	Occurrence of Numbers						Residuals				Difference R–C	
	Below x			Exactly x			Below x		Exactly x		Below	Exactly
x	C	R	P*	C	R	P	C	R	C	R	x	x
0	0	0	0	0	1	1	0	0	–1	0	0	+1
1	0	1	1	7	1	3	–1	0	+4	–2	+1	–6
2	7	2	4	6	6	9	+3	–2	–3	–3	–5	0
3	13	8	12	12	12	15	+1	–4	–3	–3	–5	0
4	25	20	27	15	18	19	–2	–7	–4	–1	–5	+3
5	40	38	46	23	19	20	–6	–8	+3	–1	–2	–4
6	63	57	65	13	20	16	–2	–8	–3	+4	–6	+7
7	76	77	82	9	9	10	–6	–5	–1	–1	+1	0
8	85	86	91	10	6	5	–6	–5	+5	+1	+1	–4
9	95	92	96	3	2	2	–1	–4	+1	0	–3	–1
10	98	94	99	2	3	1	–1	–5	+1	+2	–4	+1
11	100	97	100	0	3	0	0	–3	0	+3	–3	+3
12	100	100	100	0	0	0	0	0	0	0	0	0

* For apparent discrepancies in this column see foot-note, Table XLIV, p. 98

In Table XLV like facts are displayed with respect to the R cases on "Number" of spots. Although the residuals under "Below x" disclose a slight shifting of R above P, especially on variates *6*, *10*, and *11* (see also curves in Plate VII), this advantage is to some extent shared with C, and the difference R–C leaves but little in R's favor. Under "Exactly x" the advantages of R are not great, as may be better seen if the data are grouped:

x	C	R	P
Below *1*	0	1	1
1–3	25	19	27
4–6	51	57	55
7–9	22	17	17
10–12	2	6	1

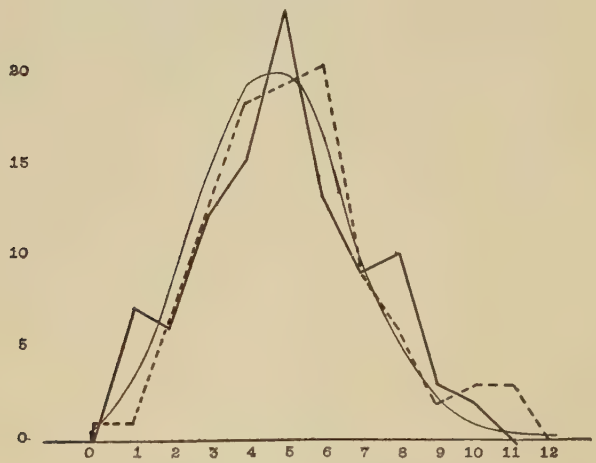

Plate VII.— R Guesses on the "Number."

100 sets, of 50, in each curve. Probable number, 5.
(Solid line, "Card Not Imaged"; broken line, "Card Imaged"; light line, Probability.)

TABLE XLVI.

Distribution of R Cases on "Suit" in 100 sets of 50.
Probability 1:4 (25%). Normal Reagents.

	Occurrence of Numbers						Residuals				Difference *R*–*C*	
	Below *x*			Exactly *x*			Below *x*		Exactly *x*		Below	Exactly
x	*C*	*R*	*P**	*C*	*R*	*P*	*C*	*R*	*C*	*R*	*x*	*x*
6	0	0	1	1	1	1	−1	−1	0	0	0	0
7	1	1	2	2	3	2	−1	−1	0	+1	0	+1
8	3	4	4	2	1	4	−1	0	−2	−3	+1	−1
9	5	5	8	10	5	7	−3	−3	+3	−2	0	−5
10	15	10	15	7	7	9	0	−5	−2	−2	−5	0
11	22	17	24	16	10	11	−2	−7	+5	−1	−5	−6
12	38	27	35	9	21	13	+3	−8	−4	+8	−11	+12
13	47	48	48	14	10	13	−1	0	+1	−3	+1	−4
14	61	58	61	11	10	12	0	−3	−1	−2	−5	−1
15	72	68	72	9	14	9	0	−4	0	+3	−4	+5
16	81	82	82	4	7	7	−1	0	−3	0	+1	+3
17	85	89	89	9	4	5	−4	0	+4	−1	+4	−5
18	94	93	93	0	2	3	+1	0	−3	−1	−1	+2
19	94	95	97	3	2	2	−3	−2	+1	0	+1	−1
20	97	97	98	1	2	1	−1	−1	0	+1	0	+1
21	98	99	99	2	1	0	−1	0	+2	+1	+1	−1
22	100	100	100	0	0	0	0	0	0	0	0	0

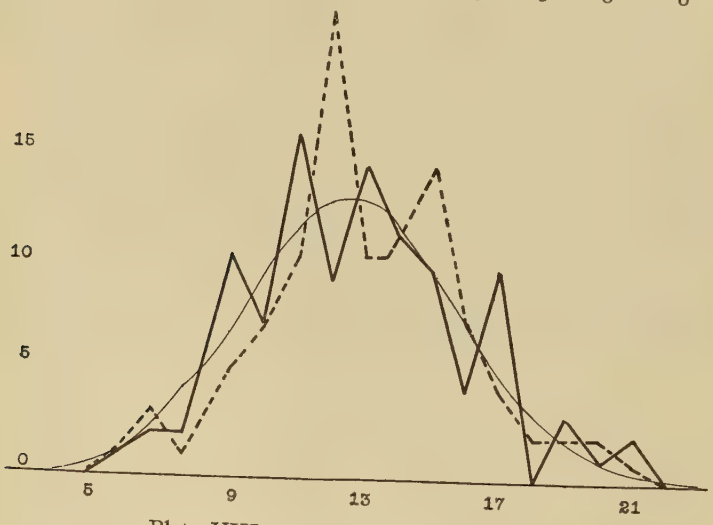

Plate VIII.— R Guesses on the "Suit."
100 Sets, of 50, in each curve. Probable number 12.5.
(Solid line, "Card Not Imaged"; broken line, "Card Imaged"; light line, Probability.)

* See foot-note Table XLIV, p. 98.

Table XLVI presents the data for R cases on "Suit," and shows amid great irregularity, best appreciated from the curves on Plate VIII, a very close approximation of R to P and C. A grouping of the "Exactly x" data gives the following view:

x	C	R	P
Below 7	1	1	1
7–10	21	16	22
11–14	50	51	49
15–18	22	27	24
Above 18	6	5	3

TABLE XLVII.

Distribution of W Cases in 100 sets of 50.
Probability 1 : 2.22 (45%). Normal Reagents.

	Occurrence of Numbers						Residuals				Difference R–C	
	Below x			Exactly x			Below x		Exactly x		Below	Exactly
x	C	R	P	C	R	P	C	R	C	R	x	x
11	0	0	0	1	0	0	0	0	+1	0	0	−1
12	1	0	0	0	0	0	+1	0	0	0	−1	0
13	1	0	0	2	0	0	+1	0	+2	0	−1	−2
14	3	0	0	0	0	1	+3	0	−1	−1	−3	0
15	3	0	1	0	1	1	+2	−1	−1	0	−3	+1
16	3	1	2	4	2	2	+1	−1	+2	0	−2	−2
17	7	3	4	8	2	4	+3	−1	+4	−2	−4	−6
18	15	5	8	7	5	5	+7	−3	+2	0	−10	−2
19	22	10	13	7	8	7	+9	−3	0	+1	−12	+1
20	29	18	20	8	12	9	+9	−2	−1	+3	−11	+4
21	37	30	29	8	11	10	+8	+1	−2	+1	−7	+3
22	45	41	39	11	12	11	+6	+2	0	+1	−4	+1
23	56	53	50	7	11	11	+6	+3	−4	0	−3	+4
24	63	64	61	7	8	10	+2	+3	−3	−2	+1	+1
25	70	72	71	8	6	9	−1	+1	−1	−3	+2	−2
26	78	78	80	5	8	7	−2	−2	−2	+1	0	+3
27	83	86	87	4	1	5	−4	−1	−1	−4	+3	−3
28	87	87	92	6	8	4	−5	−5	+2	+4	0	+2
29	93	95	95	2	2	2	−2	0	0	0	+2	0
30	95	97	97	3	3	1	−2	0	+2	+2	+2	0
31	98	100	98	1	0	1	0	+2	0	−1	+2	−1
32	99	100	99	1	0	0	0	+1	+1	0	+1	−1
33	100	100	100	0	0	0	0	0	0	0	0	0

Table XLVII and Plate IX present the data for guesses Wholly Wrong. Again, amid great irregularity there is a fairly close approximation of R to P and C. A grouping of data under "Exactly x" gives the following more regular distributions:

x	C	R	P
Below *13*	1	0	0
13–16	6	3	4
17–20	30	27	25
21–24	33	42	42
25–28	23	23	25
29–32	7	5	4
Above *32*	0	0	0

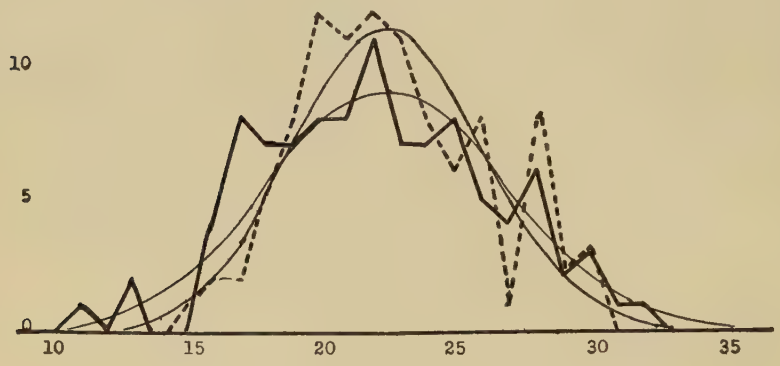

Plate IX.— Guesses Wholly Wrong.

100 Sets, of 50, in each curve. Probable number, 22.5.

(Solid line, "Card Not Imaged"; broken line, "Card Imaged"; higher light line, the theoretical curve (found by the expansion of $(q+p)^n$); the lower light line, a proximate symmetrical curve found by x/σ calculated from the two empirical curves and evaluated in a table of values of the normal probability integral. (*Vide,* Davenport: Statistical Methods, 3d ed., pp. 119 ff.).

This cursory comparison of the empirical with the theoretical distributions reveals (*a*) the irregularity of empirical distributions which may be expected when they consist of but 100 sets, and have a fairly large number of variates over which to be distributed; and (*b*) some slight advantage, which the irregularity makes difficult to see, but which the "Residuals Below x" definitely indicate, for the Regular distributions of R cases on Card, Color, Number and Suit.

Whether the advantage is significant can scarcely be estimated by visual inspection, especially since it is shared by the Control distributions almost fully in the R cases on the Card, and Suit, somewhat on

Number, and is exceeded on Color. It evidently needs statistical testing,[169] which belongs in the following section where customary formulae are applied to the empirical deviations from central measures, for the purpose of determining the reliability and the significance of the central and other single measures of the R cases reported in our tables.

Application of Usual Statistical Formulae to Central and Other Single Measures.

The single measures of the sets, with their measures of precision and their relation to the limits of chance as determined both from empirical data and from theoretical formulae, may now be inspected.

In Table XLVIII we note in line (*A*) the arithmetical means found from the 100 sets of 100 experiments each, after both the "Card Not Imaged" and the "Card Imaged" parts had been reduced to sets of 50, which agrees with the total in Table XV*a*;[170] (*B*) gives the probable number, expected by chance; (*C*) shows the deviation of the mean from the probable number; and (*D*) gives it in per cent, which is comparable with other tables and with lines (*I*), (*L*), (*M*), (*N*) and (*P*) below; (*E*) gives the customary Probable Error of the mean, (PE_M),* which indicates the distance from the mean beyond which the odds are even that the mean may fall; (*F*) gives the deviation from the mean equal to $3 \times PE_M$ beyond which the chance is 1:21 that the mean may fall; and (*G*) gives the deviation which is usually viewed as "significant," $5 \times PE_M$ beyond which the odds are only 1:1310 that the mean will fall.[171] (*E*), (*F*) and (*G*) are comparable with (*C*). (*H*) gives the Standard Deviation of the Distribution, σ † which is regarded as the best measure of variability,[172] and marks a limit beyond which 32% of all the cases fall; (I) gives in per cent the limit of 3σ, which is a rough measure of the amount of dispersion that may be expected for a normal distribution, about three cases in 1000 falling beyond it;[173] (*J*) gives Dev./σ from which (*K*) is found in a table of values giving the fraction of the area of the surface of frequency between the limits

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* $PE_M = \pm 0.6745 \frac{\sigma}{\sqrt{n}}$.

† $\sigma = \sqrt{\frac{\Sigma f \cdot \delta^2}{n}}$.

[169] *Vide, infra,* p. 108.

[170] Appendix A.

[171] Davenport, *op. cit.,* p. 14.

[172] *Vide,* Davenport, *op. cit.,* p. 16; also, Yule, *op. cit.,* p. 144.

[173] *Vide,* Yule, *op. cit.,* p. 266.

TABLE XLVIII.

R Cases. Central and limiting values reckoned upon the empirical Standard Deviation of the Distributions.

$$\left(\sigma = \sqrt{\frac{\Sigma f \cdot \delta^2}{n}} \right)$$

	Card Not Imaged.					Card Imaged.				
	Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
(*A*) Mean	1.43	25.59	4.98	12.87	22.12	1.53	24.81	5.28	13.07	22.60
(*B*) Probable Number	1.25	25.0	5.0	12.5	22.5	1.25	25.0	5.0	12.5	22.5
(*C*) Deviation	+0.18	+0.59	−0.02	+0.37	−0.38	+0.28	−0.19	+0.28	+0.57	+0.10
(*D*) Deviation in Per Cent	+0.36	+1.18	−0.04	+0.74	−0.76	+0.56	−0.38	+0.56	+1.14	+0.20
(*E*) PE_M	.083	.28	.148	.20	.29	.088	.25	.15	.20	.21
(*F*) $3 \times PE_M$	.249	.84	.444	.60	.87	.264	.75	.45	.60	.63
(*G*) $5 \times PE_M$	.415	1.20	.740	1.00	1.45	.440	1.25	.75	1.00	1.05
(*H*) σ	1.235	4.11	2.195	3.00	4.24	1.31	3.64	2.22	2.99	3.09
(*I*) 3σ, Per Cent	7.410	24.66	13.170	18.00	25.44	7.86	21.84	13.32	17.94	18.54
(*J*) Deviation/σ	.146	.143	.091	.123	.09	.214	.052	.126	.190	.038
(*K*) $2 \cdot \Sigma^{\circ}_{\delta/\sigma}$	116	114	72	98	72	169	41	100	141	30
(*L*) Dev. in Per Cent, $P = 0.76$	+0.830	+2.40	+1.48	+2.00	−2.90	+0.880	+2.50	+1.50	+2.00	−2.10
(*M*) Dev. in Per Cent, $P = 0.999$	+8.14	+27.0	+14.4	+19.7	−28.0	+8.63	+23.9	+14.6	+19.7	−20.3
(*N*) Maximum + Dev. Per Cent	+9.50	+26.0	+9.0	+17.0	−19.0	+9.50	+14.0	+12.0	+17.0	−15.0
(*P*) $L = \gamma \sqrt{\frac{2pq}{n}}$	+9.37	+30.0	+18.0	+26.0		+9.37	+30.0	+18.0	+26.0	

of 0 and x/σ.[174] The (K) values indicate the number of cases out of 1000 which may be expected to fall between 0 and plus or minus the deviations shown in (C). (L) gives the values of the deviations in (C) in per cent, which will satisfy the requirement that $P = 0.76$. (M) gives in per cent limits, $3.29 \times \sigma$, beyond which only one case in 1000 may be expected to fall; and (N) gives the maximum positive deviation in per cent, and (P) gives the theoretical limit, also in per cent, stated in the first line of Table XLI, for comparison with the empirically derived limits.

Line (C) shows how little the mean (A) deviates from the probable number (B). The deviation, in most of the cases, does not exceed $2 \times PE_M$, which is a limit beyond which the mean may be expected to fall in one case out of every 6 cases of 10,000 experiments such as ours. Only two deviations (+0.28 on the Card and +0.57 on Suit on the "Card Imaged" side of the table) approach closely the limit of $3 \times PE_M$, (F), beyond which the mean may be expected to fall once in every 22 cases. For these deviations to be "significant" they should equal at least $5 \times PE_M$, (G): that would indicate that there would be only one chance in 1311 cases for the mean to coincide with the theoretically expected number (B). Line (L) shows this limit in per cent of number of sets (100), and line (K) shows how small is the deviation of the mean from the probable number, by indicating the number of cases in a distribution of 1000 which may be expected to fall between the limits below and above the mean, equal to the deviation. So much for the relation between our central measure, the mean, and its deviation from the probable number, as revealed by the Probable Error of the mean, which is derived from the facts of the actual distribution of R cases in our 100 sets of experiments. Were there a general force beyond chance working in most of the sets for R cases, it should be detected here in the mean. In case such a force expressed itself in a few sets only, the mean might not reveal it.

Whether some of the sets exceeded chance in number of R cases, so far as the question can be settled by a central measure, must be ascertained by means of the measure of variability—the Standard Deviation (σ), shown in line (H), which fixes the negative and positive limits from the mean between which 68% of all the cases distributed may be expected to fall: 3σ, given in per cent of number of experiments (50 in a set) in line (I), is a convenient limit beyond which only 3 cases out of 1000 may be expected to fall. When the largest positive devia-

[174] Davenport, *op. cit.*, pp. 119 ff.

tion (N) is compared with (I) and (M), the latter of which gives the limit beyond which but 1 case out of 1000 may be expected to fall, it is pretty clear that the limits of distribution of the R cases in our 100 sets are quite close to what is expected of chance deviation, and that, therefore, the R cases in single sets have not been augmented by some cause besides chance, sufficient in amount to place any of them beyond the expected limit.

By comparison of (N) and (P) one can see that the theoretical deviation from the probable per cent (P) demanded of the number of R cases in a set, in order to be considered proof of some cause besides chance, lies fairly close to the maximum positive deviation, expressed in per cent (N), which we have found in our distributions of the R cases of sets. This may be regarded as justifying the use of the formulae by which Tables XLI and XLI*a* and their accompanying curves were derived, since for a set of 50, where deviations are likely to be quite erratic, our empirically derived limit of chance so closely approximates the limit theoretically derived.

One remaining single measure, deferred from preceding pages,[175] may now claim our attention: the measure of the Closeness of Fit[176] of the curves of distribution of R cases in the "Card Imaged" experiments, with their corresponding theoretical curves. For the "Card,"[177] for example, this measure is 2.9, with a probability of 0.169; which means that the fit is sufficiently close to be expected in 169 cases out of 1000, when the deviations are due to chance alone. The other measures in the following table are to be similarly interpreted:

	Δ	P
Card	2.9	0.169
Color	4.92	0.084
Number	2.89	0.760
Suit	4.08	0.274

[175] *Vide, supra,* pp. 96-97, 104-5.

[176] For Formulæ, *vide, infra,* p. 110.

[177] *Vide,* Plate V, *supra,* p. 99.

Statistical Expectation of Reagents.

As has been noted, the experience of the experimentation in the laboratory usually left the reagent's faith in the telepathic hypothesis whole. He usually discriminated from three to five degrees of certainty with which he made his guess, and since about 18% of the guesses were made with a high degree of certainty, that is, with a definite feeling that they stood a much better chance of being right than a pure guess would have, he often frankly expected statistical confirmation. Out of 70 reagents, 52 were able, after they had been given a list of the probable per cent of R cases for the various rubrics to be expected from chance alone, to express their statistical expectation in quantitative form (per cent of the whole number of experiments in a set). These estimates are put in Table XLIX*a*,[178] the footings from which are included in

TABLE XLIX.

Per Cent of R Cases expected by Reagents. (52 Reagents).

	Card	Color	No.	Suit	W	Total
(*A*) Total R Cases Expected...	319	3342	880.5	1939	1457.8	5200
(*B*) Average Per Cent.....	6.14	64.3	16.9	37.2	28.0	
(*C*) Probable Per Cent.....	2.50	50.0	10.0	25.0	45.0	
(*D*) Dev. Set of 100.....	+3.64	+14.3	+6.9	+12.2	−17.0	
(*E*) Dev. Set of 50.....	+7.28	+28.6	+13.8	+24.4	−34.0	
Minimal Signif. Dev.*						
(*F*) *a*. Set of 50.....	+9.37	+30.0	+18.0	+26.0	−36.0	
(*G*) *b*. Set of 2000.....	+1.48	+4.7	+2.8	+4.1	-7.1	
Dev. of Results:						
(*H*) *a*. Card Not Imaged.....	+0.43	+1.7	+0.7	+0.9	−1.7	
(*J*) *b*. Card Imaged.....	+0.29	−0.2	0.0	+1.2	+0.7	

By comparison of the expected Deviation, line (*E*), with the Probable Per Cent, line (*C*), and with the Minimal Significant Deviation for a set of 50 (*F*), it may be seen that the average expectation for a single set was quite close to but not above the limit of chance; but for a set of 2000 (*G*), somewhat less than the number of Card Imaged experiments in their aggregate results, it greatly *exceeds* the limit of

* From Table XLI, *supra*, p. 89.

[178] Appendix A.

chance. The relations between the expected deviations, the limit of chance, and the actual results of the Card Imaged part of these reagents' work may be more clearly perceived in Plate X, in which lines (*E*), (*G*), and (*J*) are illustrated.

The distribution of the expected values, shown in the accompanying curves (Plate XI), deviates quite widely from the distribution of the probability values.

If the measure of Closeness of Fit [179] of the expected to the theoretical curve for the "Card" may be taken as a general indication of the extent expectation exceeded probability for all the rubrics in general, we find that there are only about two chances in one septillion trials for the expectation to be fulfilled by chance.

$$(\Delta = 11.66. \quad P = 0.000{,}000{,}000{,}000{,}000{,}000{,}000{,}002{,}381).*$$

Closeness of Fit calculated in like manner for the distribution of R cases on the "Card," for all reagents when the card was imaged (see Column *C*, Exactly *x*, Table XLIII, and Plate V),[180] indicates that 169 out of 1000 curves such as ours may be expected to deviate as much as ours from the theoretical curve ($\Delta = 2.9$; $P = 0.169$).[181]

Analysis of Experience.

The records of the 10,000 introspections show that altogether guesses were made with five grades of certainty, and as a whole were distributed, in per cent, as follows:

A	*B*	*C*	*D*	Guess	Indeterminate
3.88	14.03	37.11	26.16	16.25	2.57

Four reagents out of the 100 used less than three grades, while the ma-

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$$*P = e^{-\frac{1}{2}\Delta^2}\left(1 + \frac{\Delta^2}{2} + \frac{\Delta^4}{2\cdot 4} + \frac{\Delta^6}{2\cdot 4\cdot 6} + \ldots + \frac{\Delta^{\Lambda-3}}{2\cdot 4\cdot 6\ldots\Lambda-3}\right);$$

Λ = number of classes in the distribution, and e is the base of the Napierian logarithms.

[179] Davenport, *op. cit.*, p. 24, gives it as $\Delta = \sqrt{\Sigma_y^{\delta\cdot 2}}$, in which δ_1 is the difference between the theoretical value (γ) and the observed frequency (f). of each respective class in the distribution, and Σ is the sign of summation.

[180] *Supra*, pp. 97-99.

[181] *Supra*, p. 108.

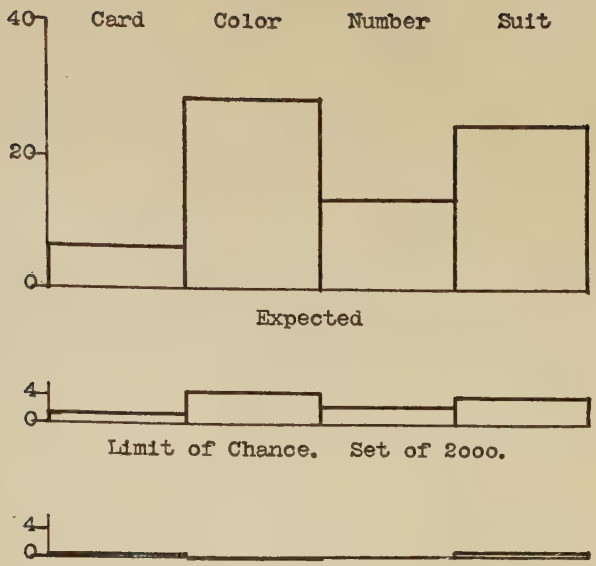

Plate X.— Deviation from Probability Expected by 52 Reagents, on Card, Color, Number, and Suit, compared with the Limit of Chance and the Actual Results.
(The values plotted in the first curve will be found in Table XLIX*a*, Appendix A.)

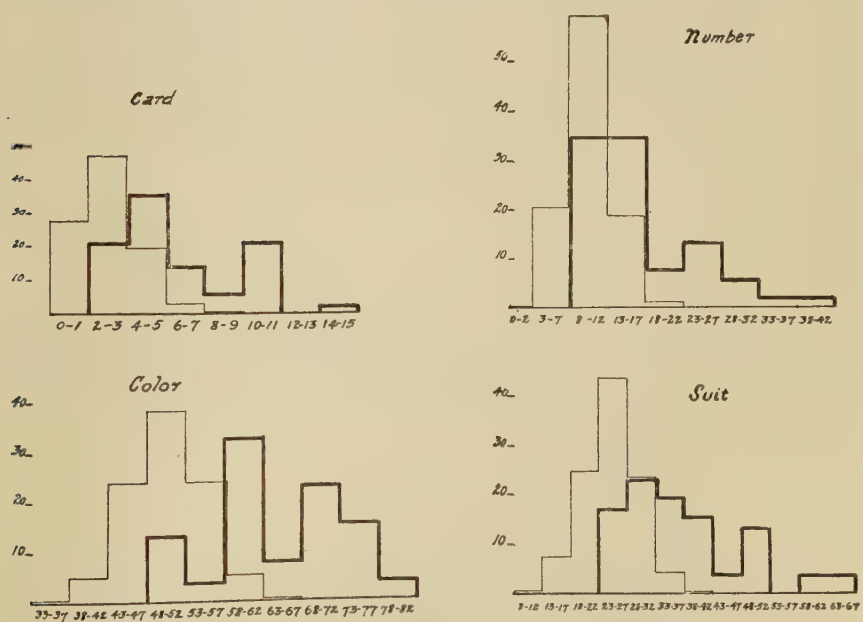

Plate XI.— Distribution of Expected Per Cents of Right Cases (heavy line) compared with Chance Distribution (light line).
52 Reagents, 5200 Experiments.
(For values in distribution, see Table XLIX*b*, Appendix A.)

jority (73) used more than three; the individual variation is shown in the following distribution:

Number of Grades used....	0	2	3	4	5
Number of Reagents.....	2	2	23	41	32

The outstanding fact in the reagent's experience of guessing cards is this variability of the certainty or confidence with which the guess is made. It no doubt constitutes a strong support for a hospitable attitude toward the telepathic hypothesis, as it does for the reagent's definite expectation that statistical verification lies in the data of his experiments. The causes for this subjective basis for the belief in thought-transference can only be found by a thorough analysis of the experience of the reagent.

His records also show that during the critical interval of the experiment, while his eyes are closed and he is passively alert for an "impression" of some card, pertinent imagery of some sort presents itself in his mind, and sometimes with great vividness and force. With respect to the mode of the imagery, individuals vary considerably. Some of the reagents were dominantly Visual (39%), some Auditory (2%), some Kinaesthetic (feeling of the organs of pronunciation) (4%); some used predominantly two modes: Visual and Auditory (19%), Visual and Kinaesthetic (13%), Kinaesthetic and Auditory (3%); while others used freely imagery of all modes (18%), or were doubtful about the imagery in which their impressions came (2%). The distribution of individuals with respect to dominant mode of imagery was:

Dominant Mode.....	*V*	*A*	*K*	*VA*	*VK*	*KA*	Mixed	Doubtful
Number of Reagents.....	39	2	4	19	13	3	18	2

The experience of the individual reagent, however, was not uniform, as in routine, but varied during his set of 100 experiments to some degree, in almost every respect covered by the classification furnished him for his introspections. Considered in the aggregate with respect to the more dominant mode of the imagery from which the guesses were made, the cases distribute as follows:

More Dominant Mode.....	*V*	*A*	*K*	None	Undetermined
Per Cent of Guesses.....	49.2	18.4	15.7	16.3	1.0

The reagent's feeling of certainty was not usually determined by the mode of his impression, however,—although there were sixteen reagents who stated that the visual mode, and five that the auditory, was preferred,—but by special intensity, quality, or behavior of the imagery

or sensations which came into his mind during the critical interval. This information is derived both from the recorded replies to the last question in the Outline for Introspections, asking why the guess was graded high, and from private interview after the reagent's experiments were over and he was requested to explain more fully his introspections.

That the experiences which contributed a high degree of certainty to the guess were unusual is indicated by the relatively small number of high grades recorded by the various reagents: 82% gave grade *A* to less than 6% of their guesses, and 55% gave a high grade (either *A* or *B*) to less than 16% of their guesses, as may be calculated from the following table, which shows the distribution, and the accompanying curves (29 reagents gave grade *A* from 1 to 5 times):

No. of times Grade was given.....	*0*	*5*	*10*	*15*	*20*	*25*	*30*	*35*	*40*	*45*	*50*	*55*	*60*	*65*	*70*
No. of Reagents who gave Grade *A*.	53	29	10	1	2	1	0	1	2	1					
A or *B*.	10	19	11	15	8	10	4	7	4	4	4	1	1	1	1

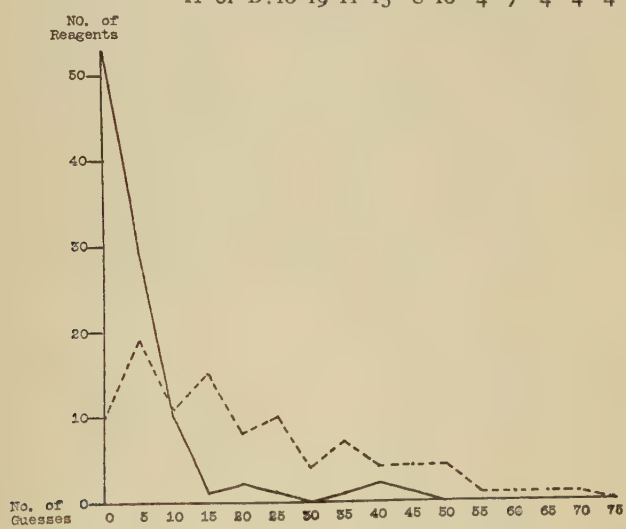

Plate XII.— Distribution of the Number of A Grades (solid line) and the Number of A and B Grades (broken line) Given by One Reagent.
100 Reagents. 1-5 plotted on 5; 6-10 on 10; etc.

Recorded interviews give the opinion of the reagent, as generalized from his experience, that his feeling of certainty depended upon—

(*1*) The strength, vividness, clearness, or distinctness of the imagery or impression;

(*2*) Its persistence, intermittence, or recurrence within the critical interval;

(*3*) Its appearance at the beginning or at the end of the interval;

(*4*) Its suddenness of appearing, "flashing out"; or its deliberate and gradual "maturing" into definiteness;

(*5*) Its apparent location; and size, if visual;

(*6*) The completeness with which the detail of the card was represented;

(*7*) Reduplication of the card, or its exclusive possession of the field;

(*8*) The impression of the card in dual or combined mode;

(*9*) A sense of reality of the impression;

(10) Special content or behavior of the imagery accepted as significant;

(*11*) A sense of the independence of the impression from mental process or volition;

(*12*) An inner feeling of certainty, not further analyzable;

(*13*) An extra-self active principle;

(*14*) Some accompanying element of experience accepted as a token of certainty, such as a vibration in the ear;

(*15*) Or merely an inference from favoring circumstances, such as freedom from distraction, a clear mind, good concentration, etc.

The recorded introspections support these generalizations:

(*1*) "Very distinct" (2:5:2),* "Very vivid" (4:7:3), "Strength of impression" (9:1:1), "Vividness" (10:4:1), "Extra clear" (11:1:2), "Extremely clear" (11:6:8), "Plain sight of card" (12:1:9), "Vivid picture" (16:9:8), "Very vivid and loud" (17:4:6), "Vivid" (18:4:5), "Impression very vivid" (19:1:3), "More plain than usual" (33:2:3), "Unusually vivid" (33:6:10), "Plainly seen" (34:3:5), "Clearly heard" (34:4:6), "Exceptionally clear" (34:5:9), "Distinctly heard" (34:6:5), "Clear vision" (35:6:1), "Appeared so clearly" (36:1:8), "It was so plain" (36:5:2), "Sounded quite sharp" (39:2:9), "Sensation of *V* and *K* so clear" (40:7:1), "Seemed shouted out at me" (86:8:10), "Very vivid" (88:7:3), "Strong (auditory) impression" (90:10:3), "Very distinct" (44:2:6), "Clear and distinct" (45:2:10), "Clearest one this hour" (41:7:4), "Saw it vividly" (46:3:5), "Very plain" (47:9:7), "More vivid impression, seemed to hear words plainly" (48:3:8), "Saw it plainly" (94:1:2), "Heard it clearly" (94:5:2), "Very clear image" (53:2:2), "Could hear it distinctly" (57:3:3), "Heard it distinctly" (61:2:4), "Force of it" (55:1:7), "Certain of judgment because it sounded so distinct and plain" (63:1:10), "Clearer picture than usual" (66:2:1), "Saw it written out and image seemed strong" (66:3:5), "Clearest picture I have got yet" (66:6:8), "Impression strong, some one speaking" (73:1:7), "I could see the card stand out sharp" (69:2:9), "Image seemed especially distinct" (79:10:8), "Felt my tongue saying, then saw the card" (29:6:3), "Hurt my throat" (6:8:10), "Felt it distinctly in throat" (13:3:7), "Was more distinct" (27:5:9), "Very clear feeling in throat" (38:1:8), "Felt strength [of impuse to speak it]" (95:5:5), "Strong impulse" (51:1:1), "Strong feeling in throat and impulse to pronounce [name of card]" (84:2:5), "Great desire to repeat 'deuce of diamonds'" (33:7:3), "Am sure I heard it" (20:5:7), "I could see it" (20:9:4), "Saw it vividly in experimenter's hand" (21:1:8), "Heard it pronounced emphatically" (21:6:7), "Seemed to see that card" (25:2:5), etc.

*The parentheses indicate that the quotation is made from the records of the 2d reagent; his 5th series, 2d experiment.

(*2*) "Because *V* and *K* persisted" (3:5:2), "Definite, persistent" (4:8:3), "Kept reappearing in mind" (25:7:6), "Impression did not change" (27:7:5), "Insistent" (27:8:4), "Persistent [auditory image]" (34:1:7), "Very persistent [visual image]" (34:1:9), "Persistency" (43:4:4), "Plainness and persistence of sound" (48:1:3), "Very persistent and clear" (53:1:5), "Very persistent" (56:2:2), "Saw clearly and continuously" (57:3:7), "Card persisted" (73:8:9), "It kept recurring" (76:3:6), "Because of the persistency after once heard" (78:1:6), "It was hard to force it out of my mind although it came the first instant" (79:9:6), "Clear and persistent" (88:4:1), "Loud and persistent" (88:9:1), "A steady recurrence" (92:7:6), "Very persistent" (93:1:3), etc.

(*3*) "Immediate" (3:8:3), "Came immediately and was very persistent" (4:2:5), "Came immediately, persistent" (4:8:7), "Kinæsthetic sensation came immediately and remained" (27:1:2), "Heard it immediately" (27:6:2), "Impression came immediately" (90:5:1; 7:10), "It came earlier than others" (41:1:10), "Clear, quick and persistent" (53:1:9), "Very quick and strong image" (53:4:6), "It entered right away and I could not get rid of it" (54:1:3), "Quick plain image of the card" (54:2:8), "Came early" (56:1:5), "Saw card quickly and clearly" (47:1:4), "Came quick and distinct" (57:2:1), "Quick clear impression" (61:10:1), "It came so strongly and quickly" (66:1:3), "Came quick; I imagined I could hear the name of card" (68:2:10), "A picture came as soon as I concentrated" (68:3:9), "Came quickly and stayed" (76:9:6), "Quicker and more definite than most" (76:8:10), "Could see it so plain and quickly" (69:2:1), "Came into my mind immediately" (70:1:2), "Picture at once" (70:4:1), "It loomed up as soon as I began; it was very vivid all the time" (82:1:7), "Came quickly and stayed" (76:6:9), "Quicker and more definite than most" (76:8:10), "Could see it so plain and quickly" (69:2:1), "Came into my mind immediately" (70:1:2; 10:2:2), "Picture at once" (70:4:1), "Very clear and quick to come to my mind" (82:2:4), "Persistency and quickness" (78:4:3), "Came quickly and clearly" (87:2:7), "Very clear and immediate" (90:1:7), "Impression came immediately" (90:5:1; 7:10), "Very quick and persistent" (94:2:9), "Quick and vivid" (94:10:5), etc.

(*4*) "At the end [of the interval] the four of spades came in quickly" (6:1:9), "Saw ace of hearts in a flash" (6:4:5), "Quickness of appearance" (12:3:10), "Quickness" (22:5:10), "Vision of card came suddenly and clearly" (25:6:6), "Because the impression came quickly" (27:2:7), "Seemed to come with a vim" (39:2:6), "Words flashed into mind" (33:5:6), "It came suddenly and clearly" (41:1:8), "It jumped into view very quick and clear" (41:2:3), "Card like a flash, and distinct" (57:1:10), "Saw clearly and suddenly" (57:7:4), "Quick clear impression" (61:1:2), "Because it came to me like a flash, and very vivid" (63:1:5), "Flashed before me, first the number, then the suit" (66:1:5), "Flashed into my mind instantly" (74:3:3), "Plain, sharp; flashed before my eyes" (69:1:4), "Had no idea of number until suddenly I wished to repeat this" (79:3:1), "Flashed up definitely" (91:5:9), "Quick and vivid" (94:10:5), "Formed slowly" (65:8:1), "Saw card slowly, but surely and clearly" (57:10:3), "This grew on me more than the others" (66:1:7), "I was sure of this, as the card came in parts, first red, and then the number and suit" (68:3:8), "Slow and distinct" (94:5:9), etc.

(*5*) "Near, big" (88:5:7), "Large and vivid" (88:7:5), "Large card plain" (91:6:2).

(*6*) Specific cases of "Completeness of detail" are all classified under the first head above, clearness, distinctness, etc. Reagent *6*, however, states that he gave grade *A* if he could count the spots.

(*7*) "No other ideas" (3:2:10), "No other card seen at all" (3:6:7), "Only card image I had" (25:7:2), "Single impression" (25:9:8), "Alone in field" (45:2:6), "Came quick and sure; no other image" (68:1:1), "Came quick and alone" (68:1:5), "I could see nothing else but this card" (68:3:4), "Stood out clearly as one especial card from a mass" (36:1:3), "Saw deck spread out on table; all vague except one" (36:2:5), "Saw little *3*'s all over" (6:1:3), "I could see about 100 of them" (32:8:2).

(*8*) "Both heard and saw card whole interval" (37:1:6), "Came so quickly and felt it in so many ways" (*VAK*) (100:3:6).

(*9*) "Seem to see the card as reality" (11:3:2), "Air of certainty, more clearly defined" (11:3:9), "Sense of reality" (16:5:6), "Very clear and vivid, good hunch" (19:4:2), "It seemed to be right" (41:2:10), "Saw and felt it as the card" (54:6:6), "Certain of judgment; card seemed so real" (63:6:10), "Positive as if I were looking at it" (91:8:8), "Very clear and real" (57:7:1).

(*10*) "Could see it pinned on wall" (11:8:3), "Mental picture of card being turned" (45:6:4), "Seemed to see the card on the bottom of the deck" (48:5:6), "Clover" (97:6:6), "Saw one bright arrow-point" (54:3:5), "Saw seven spades in a circle" (54:3:6,), "I could see the card floating through space" (68:9:5), "Saw an old friend calling it off" (70:3:3), "Definite change from what had been thinking" (76:1:9), "It was different from the others" (76:2:7).

(*11*) "Came without effort" (65:1:6). This was usually taken for granted by the reagents who recorded the detail of the impression or its specific characteristic, in the case of highly graded guesses.

(*12*) "Sure 'feeling'" (3:10:9), "Strong sense of right" (12:3:5), "Feeling of certainty" (16:1:7), "Good hunch" (19:6:7), "Feel sure it was right" (27:6:6), "Feel sure of it" (28:8:8), "Just felt that some one else was thinking of it" (79:5:9), "Feel it is right" (31:1:7), "Saw a six-spot almost at once; feel it right" (36:2:7), "I felt quite certain" (41:1:5), "I felt certain" (41:4:3), "Felt sure" (41:10:2), "Could see card plainly and felt I was right" (63:6:7), "Inward feeling" (63:7:8), "Strong feeling of correctness" (66:9:1), "Merely strong feeling" (74:1:4), "Intuition" (74:2:9), "Certainty of feeling" (74:3:2).

(*13*) "Something made me listen to my own voice repeat card" (36:2:2), "Something within impressed the card vividly on my mind" (63:3:10), "Something told me it was correct" (63:10:9).

(*14*) "Vibration felt in ear" (27:4:8), "Seemed to ring in my ears" (77:2:6).

(*15*) "Room quiet" (25:2:2), "Mind receptive" (28:4:9), "My mind was in best condition and this card loomed up as soon as I shut my eyes" (82:1:3).

There were several exceptional cases involving the recognition of a true impression of the card, such as, "Felt as if struggling to get something that I knew" (33:4:5), or "the impression 'fits right in', 'imbeds itself there' and 'shuts out other impressions'" (56); and some in which the coincidence was expected to be forced by the will, as, "I willed it to be the number" (74:1:2).

This moderate selection from the 1791 records of highly graded guesses will perhaps suffice to portray the concrete elements of experience responsible for the statistical expectation recorded some pages above. Individual differences are as wide here as they usually are found to be in mental performances. But, if one were to abstract from those differences, and seek the prime causes of confidence in guessing, he would undoubtedly find them in the unusual, or hitherto unnoticed, vividness of imagery, in its apparently independent or automatic appearance and behavior, and in equally automatic impulses to articulation. We have, then, in the quotations listed above, *experiences constituted largely of, or directly supported by, sensory and motor automatisms.* Many of them are so definite and so cogent that they will withstand the oft-repeated criticism that "you cannot get the experience of thought-transference in the laboratory." They are indistinguishable from the phrases used by both private and professional "psychics" in describing their experience, except that they are more explicit in introspective report of imagery. So far as *subjective experience* of the telepathic process is evidence for the objective existence of it, our data seem valid for testing the phenomenon whether in or out of the laboratory.

In the interest of exactness some qualification should be made as to the value of our highly graded guesses. It is true that almost all of the reagents who gave high grades of certainty to some of their guesses were in a position similar to that of Reagent 100, who said that the experience of the experiments made her feel more (than before) inclined to expect that the experimenter's part makes a difference, and that R cases will to some extent exceed chance.

But there were some exceptions: Occasionally a reagent graded "provisionally" on the basis of the vividness of his experience and, like Reagent 55, expected some excess in R cases, or like Reagent 69, considering his momentary feeling of certainty wholly unreliable as indicative of anything beyond pure chance, expected probability results.

Although the inner experience of thought-transference occurred in a substantial proportion of our experiments in the laboratory, it, like the highly graded guess which it determined, fell indifferently upon the "Card Not Imaged" and the "Card Imaged" experiments, and no more often than the low-graded guess produced an R case (see lines (*H*) and (*J*) in Table XLIX).[182]

[182] *Supra*, p. 109.

Application of Our Results to the Making of a "Mental Telepathist."

If the reader cares for a practical application of certain facts which he now holds in mind, let him consider the reagent a solid-headed man of business, his *inner experience* of thought-transference an interesting phase of his work-a-day life, and our data the events from which he draws his conclusions concerning the objective occurrence of thought-transference; this "mental telepathist," especially if he is inclined to make experimental mysticism his avocation, will almost necessarily, unless he holds rigidly to scientific procedure and mathematics, establish his faith upon the facts of experience—partial, fatally selected, experience. He finds that his feeling correlates with coincidence; apart from the many partial successes, he may list:[183]

(1) Hits on Cards Imaged.

1. (79:10:8)	"Image seemed especially distinct"	B1S	1
2. (78:2:8)	"Could hear card named persistently"	R7D	5
3. (32:8:2)	"I could see about 100 of them"	R1D	5
4. (66:6:8)	"Clearest picture I have got yet"	B6S	5
5. (66:9:1)	"Strong feeling of correctness"	R2D	1
6. (68:5:8)	"The name of the card came to me quick"	B5S	5
7. (68:10:6)	"I could hear the name of the card called"	R6H	1

He has the acknowledgment of the second person, corresponding to our experimenter, that these received and tabulated thoughts were actually his thoughts at the time of the inner experience, (Odd die-spots).

These hits give him much gratification and are frequently recalled and related. They furnish strong items for generalization.

During his search for truth he has made also another list:

(2) Hits on Cards Not Imaged.

1. (32:9:3)	"Awful plain"	R5H	4
2. (37:1:7)	"Heard in distant voice"	R9D	2
3. (90:7:10)	"Came immediately"	R6D	2
4. (63:3:10)	"Something within impressed the card vividly on my mind"	B6C	6
5. (66:10:4)	"Especially good image"	B7S	4
6. (70:1:2)	"Came into my mind immediately"	R2H	6
7. (70:4:7)	"Immediate picture"	R4H	2

[183] To interpret the first line: Reagent *79*, in the 8th experiment of the 10th series of his set, gave as a reason for the high grade of certainty with which he made his guess the phrase within the quotation marks. The card held by the

These indeed have been coincidences, and the second party acknowledges that those specific facts were in his mind "about" the time of the inner experience. They also are hits, and are also recalled and reported with satisfaction. The fact that the coincidence involved the content of but one mind ("Card Not Imaged," as indicated by the Even die-spots) is not easily checked up or, indeed, brought to notice at all.

These are the oases of adventure in the occult. Hundreds of partial successes, bewildering in their detail, are also recorded. Yet many other facts have been noticed but have not been often recalled, much less listed;

(3) Complete Misses.

1.	(78:4:3)	"Persistency and quickness"	R6H 3
2.	(79:5:9)	"Just felt that some one else was thinking of it"	R7D 6
3.	(79:6:5)	"Impression was very vivid"	B8C 5
4.	(79:9:6)	"It was hard to force it out of my mind although it came the first instant"	R2H 1
5.	(33:5:6)	"Words flashed into my mind"	B6C 1
6.	(33:7:3)	"Great desire to repeat 'deuce of diamonds'"	R2D 3
7.	(34:6:5)	"Distinctly heard"	B6C 4
8.	(36:2:7)	"Saw a 6-spot almost at once; felt it right"	R6D 2
9.	(87:2:7)	"Came quickly and clearly"	R6H 2
10.	(41:1:5)	"I felt quite certain"	B1S 6
11.	(41:1:8)	"It came suddenly and clearly"	B9S 3
12.	(41:2:10)	"It seemed to be right"	R2H 4
13.	(41:6:6)	"I felt very certain"	R3H 3
14.	(41:10:2)	"Felt sure"	R9D 2
15.	(43:9:7)	"Inward feeling"	B5C 3
16.	(47:9:7)	"Very plain"	B4C 6
17.	(63:6:7)	"Could see card plainly and felt I was right"	R10H 3
18.	(63:10:9)	"Something told me it was correct"	B2S 2

etc. for 250 cases. (To balance the other lists.)

These are not hits and are not interesting; their very frequency seems to make them common and cheap; they never attain sufficient weight to qualify a generalization, except the suggestion that the conditions essential for telepathy are unknown and can, therefore, be fulfilled only by accident. Our mental-telepathist has *experienced* telepathy; negative cases prove nothing.

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experimenter was Black, the Ace of Spades, which, as indicated by Die-spot 1, was held during the critical interval in sharp visual impression. The black-face type indicates that the guess was correct with respect to all the elements of the card—Color, Number, and Suit.

Then there is this remaining list of often-noticed, but not sufficiently weighed, cases:

(4) Complete Chance Hits.

1.	(1:1:4)	"Pure Guess"	R7D	1
2.	(1:1:7)	" "	B8C	5
3.	(5:4:1)	" "	R5H	2
4.	(5:5:2)	" "	R2D	5
5.	(5:5:4)	" "	B5S	4
6.	(17:7:1)	" "	B9S	2
7.	(21:9:5)	" "	B9C	4
8.	(25:1:1)	" "	R9D	3
9.	(25:2:9)	" "	R5D	6
10.	(25:5:5)	" "	B10S	4
11.	(27:10:4)	" "	R4H	3
12.	(32:10:5)	" "	R3H	6
13.	(37:3:3)	" "	B4S	6
14.	(40:6:4)	" "	B4S	3
15.	(40:6:7)	" "	R4H	1
16.	(42:6:4)	" "	R5H	3
17.	(48:6:7)	" "	B6C	2
18.	(50:1:9)	" "	B1C	2
19.	(50:7:3)	" "	R10H	1
20.	(59:2:7)	" "	B7C	1
21.	(60:5:3)	" "	B5S	2
22.	(60:10:10)	" "	R9H	3
23.	(78:7:8)	" "	R4D	5
24.	(85:6:1)	" "	R7H	6
25.	(91:6:8)	" "	R1D	4
26.	(98:3:5)	" "	B6S	3

There is no "inner experience" of coincidence here; they are indifferent facts, these chance hits; coincidence, of course, must happen by chance quite often. Our searcher for truth observes with a glance and passes to the enjoyment of recalling "incontestable" cases, validated by "inner experience."

The reader should not infer, from this application of our experimental results to the normal development of a "Mental-Telepathist," that an objective thought-transference is to be altogether discredited. There are quite a few good men and true who suspect that it is a fact of nature. He should, rather, especially if he is a "Mental-Telepathist," recognize in the method of investigation exemplified in these experiments a way in which telepathy may be indisputably brought to light, and then suspend his judgment until verifiable proof is produced. Discredit is, indeed, thrown upon the telepathic hypothesis by proponents who accept "selected" cases as proof. "Selection" is one of some half-dozen serious errors which cloud the evidence for telepathy.[184]

[184] *Vide*, footnote 125, p. 50.

Corneal Reflection.[185]

In order to have some sets that would illustrate the distribution of R cases when a known cause operates with chance in a greater or less degree, it was arranged for one group of students to perform the experiments in Card-Guessing according to a change in method which put the reagent in possession of a reading telescope through which upon occasion he could see the reflection of the card upon the cornea of the left eye of the experimenter. The reagent faced the experimenter across a table 1.2 meters in width, and was securely screened from him by black cardboard into which the end of the telescope was fitted. Before the experimenter tapped the signal announcing the beginning of the critical interval he adjusted his left eye into alignment with the objective lens of the telescope through a large lens mounted 10 cm. from his face. He followed the same procedure that the other experimenters followed, in the conduct of the experiments, except that when a 1-spot or a 5-spot face was thrown by the die, instead of holding the card drawn in his hand he placed it in a rack mounted at the left of the large lens. Since the light from the windows at his back fell freely upon the face of the card, and his eye was in shadow, a very clear image of the card (about one-fifth diameter of the card in size) could be got by the reagent in case he could adjust the focus of the telescope before the end of the interval. The reagent, being in ignorance of the control of the experiment, was compelled each time to focus his instrument and help out his guessing as best he could.

Five sets were obtained, and the results are displayed at the end of Table XIII (C);[186] deviations from probability in Table XIV (C);[187] R cases and deviations of Set *V* on die-spots are shown in Table XIX.[188] The results of this set were used to illustrate the method of searching for extra-chance causes of R cases.[189]

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[185] That Corneal Reflection has played a rôle in experiments in thought-transference, suggested this particular extra-chance cause. *Vide, infra,* pp. 174, 214; *cf.,* also, Thomas: Thought Transference, 1905, p. 28; and Podmore: Apparitions and Thought-Transference, 1894, p. 12.

[186] *Supra,* p. 62.

[187] *Supra,* p. 63.

[188] *Supra,* p. 68.

[189] *Supra,* pp. 67 ff.

The aggregate results are distributed over the die-spots in the following table:

TABLE L.

Corneal Reflection. R Cases distributed over the Die-Spots.

	Card Not Imaged						Card Imaged						
Die	Card	Color	No.	Suit	W	Tot.	Die	Card	Color	No.	Suit	W	Tot.
2	3	32	12	22	37	76	*1*	64	98	88	70	2	101
4	3	44	10	20	29	77	*3*	5	41	11	19	42	86
6	1	42	10	22	43	92	*5*	43	63	53	50	4	68
Totals	7	118	32	64	109	245		112	202	152	139	48	255

Per Cent.

Die	Card	Color	No.	Suit	W	Die	Card	Color	No.	Suit	W
2	3.95	42.1	15.8	29.0	48.7	*1*	63.4	97.1	87.2	69.4	2.0
4	3.90	57.1	13.0	26.0	37.6	*3*	5.8	47.7	12.8	22.1	48.8
6	1.09	45.6	10.9	23.9	46.7	*5*	63.2	92.7	78.0	73.5	5.9
Totals	2.86	48.1	13.1	26.1	44.5		43.9	79.1	59.5	54.5	18.8

Deviation from Probability, Per Cent.

Die	Card	Color	No.	Suit	W	Die	Card	Color	No.	Suit	W
2	+1.45	−7.9	+5.8	+4.0	+3.7	*1*	+60.90	+47.1	+77.2	+44.4	−43.0
4	+1.40	+7.1	+3.0	+1.0	−7.4	*3*	+3.30	−2.3	+2.8	−2.9	+3.8
6	−1.41	−4.4	+0.9	−1.1	+1.7	*5*	+60.70	+42.7	+68.0	+48.5	−39.1
Totals	+0.36	−1.9	+3.1	+1.1	−0.5		+41.40	+29.1	+49.5	+29.5	−26.2
Limit of Chance Deviation.....							±4.15	±13.3	±8.0	±11.5	±13.2

Here some cause operated more or less effectively in one-third of the experiments, or in two-thirds of the "Card Imaged" experiments. By comparison of the deviations in Table XIV (C)[190] with Table XLI[191] it may be seen that on the "Card" and on "Number" (Card Imaged) the deviations in all sets are above the theoretical limit of chance; on "Color" and on "Suit" Sets *II, III,* and *V,* fall below, the reagents having found some difficulty in determination and having to guess more often. If the aggregate results are compared with the theoretical limit of chance (in the above table, last two lines), however, all deviations on the "Card Imaged" side exceed the latter considerably.

Two reagents graded their judgments: Reagent *I* gave 5 *A*'s, 6 *B*'s,

[190] *Supra,* p. 63.

[191] *Supra,* p. 89.

7 *C*'s, and 2 *D*'s, all on die-spots *1* or *5*, with the following result in per cent of R cases:

Grade	Card	Color	No.	Suit	W
A	100	100	100	100	0
B	100	100	100	100	0
C	43	100	100	43	0
D	100	100	100	0	0

Reagent *V*, who had greater difficulty with the apparatus, gave 11 high grades, the aggregate R cases of which exceeding the probable per cent as follows:

	Card	Color	No.	Suit	W
Deviation, Per Cent.....	+24.5	+41	+35	+30	−36

This material is of service principally in illustrating our method of statistical inquiry, which it vindicates in showing that confident judgments fall upon odd die-spots and correlate with R cases and that the latter also are correlated.[192]

Conclusion.

The majority of students in our large general courses in psychology are favorably disposed toward the telepathic hypothesis. The results of 10,000 guesses of cards made by 100 reagents selected from these classes because of their favorable attitude, working in laboratory partnership with experimenters likewise selected, show

(1) That 73% of the reagents graded some of their guesses high, indicating that they expected those particular guesses to stand a much better chance of being right than "pure guesses" would stand;

(2) That 18% of all the guesses were graded high;

(3) That causes of the high grades lie in various forms of sensory and motor automatisms which in their concrete expressions testify to the occurrence of the "inner experience" of thought-transference here in our laboratory;

(4) That various statistical treatments of the data fail to reveal any cause beyond chance operating for R cases, whether the guesses are graded high or low;

[192] An amusing incident occurred when Experimenter *IV* returned his results, and since it illustrates the power of a few chance hits to break down scientific reserve it may be worthy of record. He said that after the experiments were concluded he learned by checking up results that the reagent's telescope often revealed in his eye the card he was thinking of (die-spot *3* determining the concealing of the card and holding it in kinaesthetic imagery), and thought there must be some way by which his imagery escaped through his organs of vision.

(5) That the "inner experience" of thought-transference occurs indifferently in control and regular experiments and with Wrong and Right cases;

(6) That the determinations of values derived from theoretical probability are closely approximated by those derived from inductive probability, as afforded by our Control experiments, and are as satisfactory as the latter for comparison with the values derived from the Regular experiments for the purpose of testing the latter for an influence beyond chance working for R cases;

(7) That the deviations of neither the single values nor the distributions of the Regular experiments exceed chance, either theoretical or empirical; and consequently

(8) That no trace of an objective thought-transference is found either as a capacity shared in a low degree by our normal reagents in general (Richet's "Suggestion Mentale") or as a capacity enjoyed in perceptible measure by any of the individual normal reagents.

These negative results, together with those found by the American [193] and the British [194] societies for psychical research, may be taken to indicate that the normal person who has telepathic power is relatively rare, as has been surmised by Sir Oliver Lodge,[195] and that the telepathic experience, if it occurs at all, probably occurs only when the mind is in a "critical" state—a principle suggested by James.[196]

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[193] "These returns [of the experiments conducted under the guidance of the American Committee] have been subjected to a careful mathematical analysis by Professors Peirce and Pickering. A study of their special report on this subject (Appendix B., pp. 17-34) shows that the general result of these experiments is, at present, unfavorable to thought-transference as a power belonging to mankind in general." (Commitee on Thought-Transference, June 4, 1885, *Proceedings Am. S. P. R.*, Series 1, 1:8).

[194] *Vide,* quotation from Thomas, and from the *Journal S. P. R., supra,* pp. 23, 26; *cf.,* Thomas: Thought Transference, pp. 180 ff.

[195] "Clearly, most persons are opaque to telepathic impulses." Lodge: The attitude of science to the unusual. *The Nineteenth Century and After,* February 1909, 65:215.

[196] "Professor Richet's supposition that if the unexplained thing called thought-transference be ever real, its causes must, to some degree, work in everybody at all times (so that in any long series of card-guessings, for example, there ought always to be some excess of right answers above the chance number) is, I am inclined to think, not very well substantiated. Thought-transference may involve a critical point, as the physicists call it, which is passed only when certain psychic conditions are realized, and otherwise not reached at all—just as a big conflagration will break out at a certain temperature, below which no conflagration whatever, big or little, can occur."—JAMES: Address by the President. *Proceedings S. P. R.*, 1896-97, 12:3-4.

SERIES II. REAGENTS "SENSITIVE."

Series II of this investigation was undertaken for the purpose of determining by like methods how the results from "psychics," or persons reputed to be "sensitive" to telepathic or clairvoyant impressions, would differ from those obtained from normal reagents. The experiments followed the same general procedure, the card being drawn at random from a constantly reshuffled pack of 40, and its treatment being determined by the numbers cast by a die. During the critical interval the card was held unknown to the experimenter if an even number had been cast, entertained in keen visual perception upon the casting of die-spot *1*, in keen kinaesthetic imagery (of the vocal organs) upon the casting of die-spot *3*, in combined visual perception, kinaesthetic imagery, and auditory imagery, upon the casting of die-spot *5*. The writer acted as experimenter (agent) (except for Sets *8* and *9*) and recorded the introspections of the reagents (percipients) which his questions prompted; an associate recorded the judgments or guesses.

The Reagents.

The 10 reagents were "psychics." One was a student, who, however, has had remarkable psychical experiences. The others lived in cities or towns within a hundred miles of the University. Three were private persons who possessed sensory and motor automatisms regarded as psychical. The first five were spiritistic mediums. The work of the "10th psychic" was in reality done by five additional spiritistic mediums, four of whom were unable to complete the set of experiments, and an automatic writer. The attitude of all the psychics toward the telepathic hypothesis was favorable: they had no opinion upon it; they had knowledge. The mediums do not belong to the class of clairvoyants who establish offices in the business district of the city, display ostentatiously a certificate of ordination into an hypothetical spiritualistic church, and by trick and device prey upon the public. They have a sincere and an abiding faith in their mediumship, which they practice semi-professionally, bringing consolation to the stricken, peace to the troubled, answers to the questioning. They were somewhat more than interested in a decisive method of portraying some of the real facts in their experience, especially before the eyes of science, and gave their

time and effort to the research without pay.[197] It should be noted that they are mothers of families, most of them own their homes, some of them are known in the literature of psychical research, several of them have traveled abroad and appeared before investigating bodies, and at least one of them has given séances before courts in Europe. One of them appeared before the Seybert Commission of the University of Pennsylvania.

The Results.

The gross number of R cases (right judgments) is given in Table XIII (*B*);[198] the deviations of the sets, after reduction to sets of 50, are shown in Table XIV (*B*);[199] and the deviation of the per cent of R cases, for the series, is shown in Table XVII.[200] The distribution of R cases over the die-spots, the per cent of R cases, and the deviation of the per cent of R cases from the Probable per cent, are given in

TABLE LI.

Psychics. Distribution of R Cases over the Die-Spots.

	Card Not Imaged							Card Imaged					
Die-Spot	Card	Color	No.	Suit	W	Tot.	Die-Spot	Card	Color	No.	Suit	W	Tot.
2	4	77	12	42	75	156	*1*	8	100	21	46	85	195
4	3	75	16	47	65	150	*3*	8	78	21	41	67	154
6	4	80	15	41	74	164	*5*	2	99	15	51	72	181
Tot.	11	232	43	130	214	470		18	277	57	138	224	530
	Per Cent of R Cases.												
2	2.6	49.4	7.7	26.9	48.1		*1*	4.1	51.3	10.8	23.6	43.6	
4	2.0	50.0	10.7	31.3	43.3		*3*	5.2	50.6	13.6	26.6	43.5	
6	2.4	48.8	9.1	25.0	45.1		*5*	1.1	54.9	8.3	28.2	39.8	
Tot.	2.34	49.3	9.1	27.6	45.5			3.40	52.3	10.8	26.2	42.3	
	Deviation from the Probable Per Cent.												
2	+0.1	−0.6	−2.3	+1.9	+3.1		*1*	+1.6	+1.3	+0.8	−1.4	−1.4	
4	−0.5	0.0	+0.7	+6.3	−1.7		*3*	+2.7	+0.6	+3.6	+1.6	−1.5	
6	−0.1	−1.2	−0.9	0.0	+0.1		*5*	−1.4	+4.9	−1.7	+3.2	−5.2	
Tot.*	−0.16	−0.7	−0.9	+2.6	+0.5			+0.90	+2.3	+0.8	+1.0	−2.7	

*This deviation of the per cent of the total R cases from the probable per cent corresponds to the last line in Table XVII, *supra*, p. 65.

[197] The writer wishes to acknowledge here the courtesies extended to him, both by these "psychics" and by the members of the California Psychical Research Society. The president of the Society performed a valuable service in making appointments and in recording judgments in the experimentation; and the laboratory of the Society supplied a real need.

[198] *Supra*, p. 62. [199] *Supra*, p. 63. [200] *Supra*, p. 65.

The deviations from the probable per cent in this table do not indicate an obvious advantage of any odd number of die-spots over another, but they reveal some advantage of the odd over the even, which is somewhat accentuated if we segregate the professional from the private psychics:

Deviations of Per Cent of R Cases from the Probable Per Cent.

	Card Not Imaged					Card Imaged				
	Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
Professional	−0.85	−0.2	−1.4	+0.5	−0.2	+2.17	+2.9	+1.7	+1.8	−3.0
Private	+0.58	−1.1	−0.3	+5.0	+1.3	−0.89	+1.6	−0.1	+0.3	−2.5

Should this advantage prove significant we shall at last have found thought-transference among the professional psychics.

The deviations of the total in Table LI are comparable with those given in Table XVII,[201] with which they may be compared. The +0.90 on "Card" is less than half the corresponding deviation (+2.23) of the "Card Not Imaged" experiments of the 7th 1000; and the remaining deviations are each exceeded by their corresponding deviations in the same 1000, which are regarded as empirical chance deviations. If we compare the deviations with the theoretical limit of chance deviation, as given for sets of 500 in Table XLI [202] (+2.96, +9.5, +5.7, +8.2), we again find them much exceeded.

The deviations of aggregated results of the professional psychics must be estimated in terms of a set of 250, for comparison with empirical and theoretical chance deviations. If we find the deviations of the per cent of R cases from the Probable per cent for two consecutive groups of five entries in Table XIII,[203] on the "Card Not Imaged" side, say sets *61-65*, and *66-70*, we shall then have some empirical chance deviations comparable to the deviations of the professional psychics; and if we add, in the lower line, the theoretical limit of chance deviation in a set of 250 (from Table L),[204] we get the following table:

Deviations in Per Cent.

	Card	Color	No.	Suit	W
Sets *61–65*	+2.54	+4.2	+2.4	+5.0	−3.4
Sets *66–70*	+1.93	+2.0	+3.6	+5.8	−1.0
Professional Psychics	+2.17	+2.9	+1.7	+1.8	−3.0
Theoretical Limit	±4.15	±13.3	±8.0	±11.5	±13.2

[201] *Supra*, p. 65.
[202] *Supra*, p. 89.
[203] *Supra*, p. 60.
[204] *Supra*, p. 122.

In each rubric is the psychics' deviation exceeded by empirical chance deviation, as well as by the theoretical limit of chance deviation. Then, if we turn back to the Psychics' R cases (in Table XIII*a* [*B*],[205] where they are reduced to sets of 50) and compare them with the distribution of chance occurrences of R cases shown in Plates V ff.,[206] we may determine just how likely they are to occur by chance. In the following table

R = the largest number of R cases made in a set (of 50) by a psychic.

x = the corresponding deviation from the probable number.

σ = the Standard Deviation, or index of variability, of the Distribution of R cases in the "Card Not Imaged" experiments of the Normal Reagents; it is calculated from the probable number, not from the mean.

$\frac{x}{\sigma}$ = the "Index of Abmodality" (*Vide,* Davenport, *op. cit.,* p. 23).

$\Sigma_{x/\sigma}^{+\infty}$ = the number of cases in a distribution of R cases resulting from chance which lie in the positive side of the distribution farther from the probable number than the given deviation does.[207] Total equals 1000 cases.

P_1 = Inductive Probability, from the Tables XLIII-XLVI,[208] Column *C,* Exactly x, indicating the number of cases in 100 that x was exceeded by, in our empirical chance series.

P = Theoretical Probability,[209] showing the number of cases in 1000 that x may be expected to be exceeded by, in positive chance variations.

TABLE LII.

The Largest Positive Deviations of the Professional Psychics, related to the Normal Distribution of R Cases calculated from empirical and theoretical chance.

	R	x	σ	$\frac{x}{\sigma}$	$\Sigma_{x/\sigma}^{+\infty}$	P_1	P
Card (Reagent *3*)	4	+2.75	1.22	2.25	12.	2.	7.
Color (Reagent *5*)	29	+4.0	4.11	0.90	184.	16.	129.
Number (Reagent *2*)	8	+3.0	2.16	1.39	82.	5.	79.
Suit (Reagent *5*)	18	+5.5	3.14	1.75	40.	6.	36.

This table shows that the largest positive deviation in number of R cases on the "Card," (+2.75%), made by Reagent *3*, in a set of 50 experiments, may be expected to be exceeded by chance in 12 out of 1000

[205] Appendix A.

[206] *Supra,* pp. 99 ff.

[207] These values are derived from a table of values of the normal probability integral (Davenport, *op. cit.,* pp. 119 ff.).

[208] *Supra,* pp. 97 ff.

[209] Found from Davenport's table of values of the probability integral for x/σ (*op. cit.,* pp. 119 ff.), the theoretical value of σ being: $\sigma = \sqrt{pqn}$, (*Vide,* Yule: An Introduction to the Theory of Statistics, 1916, p. 262.)

sets; that it was exceeded twice in the distribution of R cases in 100 sets of "Card Not Imaged" experiments by Normal Reagents;[210] and that, theoretically, it may be expected to be exceeded by chance in 7 out of 1000 sets.

It is evident that no extra-normal capacity for thought-transference, or for lucidity, is perceptible in the results of the individual professional psychics.

Remarks made occasionally by the psychics raised the question of the possibility of a retarded influence which would show in the coincidence not of the card and judgment of a given experiment but of the card of one experiment and the judgment of the following experiment or the second following experiment; or of a capacity for prescience which would be shown in the coincidence of the card of a given experiment and the judgment of a preceding experiment. (In case the "forces" controlled the experimenter in drawing the card, as a prominent psychic suggested, the statistical equivalent of prescience would be revealed in the results.)

These hypotheses can now be tested. R cases were tabulated, and the following deviations from the probable per cent found:

TABLE LIII.

Psychics. Coincidence between the Card Drawn and the Preceding, Succeeding, and Second Succeeding Judgments. Deviations from the Probable Per Cent.

	Card Not Imaged					Card Imaged				
	Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
Preceding Judgment..	+0.28	−2.7	+0.8	−0.8	+2.3	+0.88	+0.8	+1.6	+2.4	−0.7
1st Succeeding " ..	−0.67	−1.8	+0.3	−1.4	+1.5	−0.25	−3.5	−1.4	−1.6	+3.6
2d " " ..	+0.28	+1.8	−0.8	+4.1	−1.5	−0.62	−0.5	−2.5	+1.1	+1.3

Coincidences—	First 50 Experiments.					Second 50 Experiments.				
Between Guesses.....	.0.30	+3.8	+3.0	+1.0	−5.6	−0.70	−0.6	−1.6	+0.2	+1.0
" Drawings ...	+0.89	+0.2	+1.2	+0.2	+0.6	−0.42	+0.7	−1.3	+2.6	−0.5
Limit of Chance Deviation.....						±4.15	±13.3	±8.0	±11.5	±13.2

The size of the deviations, fairly uniform with that of coincidences between guesses, or between drawings of different sets, and in no case equal to as much as half the limit of chance deviation, supports no other inference than that chance has not been interfered with in the occur-

[210] *Vide*, Table XLIII (p. 97), column *C*, "Occurrence of Numbers, Exactly *x*."

rence of R cases, and, consequently, that neither tardy telepathic influence nor capacity for prescience is apparent in our results.

It is altogether probable that the results of the psychics differ in no essential respect from those of normal reagents or from either empirical or theoretical chance, and that neither telepathic nor clairvoyant capacity has been revealed, whether such capacity is regarded as shared by the psychics in general or as possessed by an individual, especially sensitive, psychic.

Should the consistency of the positive deviations of the professional psychics over the probable per cents of R cases be regarded as indicative of a small degree of telepathic capacity, the way remains open for conclusive proof: a continuation of experiments in sufficient number to lower the limit of chance deviation below the positive deviations.

Psychological Analysis of Experience.

The Professional Psychics.

Reagent *1* gave her judgments upon three kinds of impressions, one Visual, and two Auditory. The visual consists of clear imagery of the card located before the eyes about reading distance; the first auditory consists of a "voice speaking in the head." This voice is not her own but that of a young girl about 16 years of age, and the psychic is inclined to identify it as that of "A——,"[211] a "control" who has been with her, in psychic manifestations, about four and a half years. The second auditory impression is described as a silent voice, which, however, is not accompanied by sensations in the throat. The work was done in the natural state involving the same processes as used in "circle" work. She also works in trance, getting raps, tipping, automatic voices, etc. She is inclined to place less confidence in her work done in the normal state.

Reagent *2* received visual and auditory impressions of a quite variable nature. Sometimes she saw the card clearly; sometimes luminous imagery introduced the card imagery, as "Saw (clairvoyantly) two bright spots, in them two hearts" (2:1),[212] or the light appeared in the middle of the card in blinding intensity so that at first the spots could not be clearly seen (10:2). Sometimes the lights were all that were seen (6:5). After 1:6 she described in detail the features of an old man who was holding a card; and after 4:10 she described a heavy-set,

[211] The designations of these controls have no orthographical relation to the real names, since it is intended that the psychics remain incognito.

[212] Second series of 10 experiments; first experiment.

whiskered man, who was holding the experimenter's hand trying to guide it in drawing the card. The room, she said, was full of spirits, and progress with the cards was impeded. The auditory impression is like a whisper, except that the volume is great; is like the sound of the sea-shell. This voice sometimes immediately after a judgment was given said "wrong," and the psychic was inclined to think that the voice was right but could not herself be positive (3:1. It was right for color and suit). Curiously the card in 1:4 which was "known" was wholly missed, and the next one, 1:5, which drew the only "pure guess" in the 100 experiments, was completely hit. The psychic has lived in her own home for years, has grandchildren, and has held sittings weekly for twenty years. She is intelligent, and frankly enthusiastic at the prospect of finally proving something.

Reagent *3* was able to give her judgments grades, according to the vividness of the impression, but, unfortunately, many were low; and there were many pure guesses:

A	*B*	*C*	*D*	*PG*
1	14	16	13	56

The 15 highly-graded judgments fall upon 10 "Cards Not Imaged" and 5 "Cards Imaged," and yield in R cases, Card 0, Color 60%, Number 0, Suit 33%, and Wrong 40%. Card imagery was uncertain and capricious, showing colored backs, only parts of the face, the card in motion, blank cards, etc., consequently many judgments were satisfactory only in a single particular. Gathering such judgments together we have [213]

1:2	Black, spots blurred, card moving	**B8S**	3
2:8	Pure guess except as to color, seemed to look over experimenter's shoulder	**R2H**	3
3:1	Saw spade, but the spots were out of order	**B3S**	6
3:3	Red; 3 spots on left, 1 on right	**R7H**	5
5:10	Only "Spades," "3" is a guess	**R6D**	6
6:7	"Clubs" only	**B2S**	6
9:4	Guess on number	**B7S**	1
10:10	Guessed number	**R7D**	1

[213] To interpret first line: In second experiment of first series (of 10 experiments) the card drawn was Black, the Eight of Spades; it was held in Kin. imagery (Die-spot *3*); the psychic's guess was right as to Color (black-face type); her experience is indicated in the phrase recorded after the experiment-number.

A comparison of the imagery with the cards drawn and with the die-spots shows four hits out of the eight cases, and the complete card guessed was not imaged by the experimenter.

Impressions came mostly in visual form; sometimes in impulse to speak; twice the name was simply "impressed," once yielding a complete miss, once a complete hit; and once the name of the card came as a whisper (9:3, *B5S*, card held in visual impression was *B4S*). There was much other imagery, chiefly visual, not relating to the cards: once (9:9) the 2-spot face of the die was seen when the 1-spot had been thrown; other imagery was a side view of a life-size statue, sitting position (2:6), a big bronze hinge (2:9), a cabinet 15 inches square of dark metal and encircled with a one-half-inch band of light (2:10), a white disc (3:5), a United States flag at half mast and a crowd of people (3:6), the figure of Lincoln (3:6), searchlight and ship and a wreath of white roses (3:7), a white upright panel, 5 feet by 2 feet (3:9), a brass jardiniere and a papyrus plant (3:10), a blue bird (4:2), an arch (4:3), a mountain waterfall (5:2), a flood with furniture in water in Ohio (5:3), ice and W. T. Stead (8:9), rugged ocean shore (8:10), Prince Albert monument (9:2), Minnehaha Falls frozen (10:6).

The psychic is recognized by those having discriminating judgment in such matters as one of the most reliable mediums in the country, and has held select circles for many years.

Reagent *4* gave judgments from visual impressions; her guide "B——" shows her the card; very few pure guesses were made. The work of the experimenters was punctuated by communications from "B——", who longed for loftier service. This interesting personality informed us that she was "American born East" and had been "in spirit long time." To show us her power over space she paid a swift visit to the moon and in a moment reported that it was very cold there, an atmosphere different from ours, no wind, heavy-like fog but clear; there were great crevices and a huge crater in which she saw blue mineral. She impelled her medium, upon another occasion, to give the following message: "I see two little men, one signing his name to a paper; an 'S' about it; a little trouble." Thinking, perhaps, that some service of a more definite nature might be offered, she invited the experimenter to ask for any information about affairs in which he was interested; and in reply to the question "Will the new course continue next year?" she advised to "go ahead," but intimated that there might be financial trouble, a dark-haired man was jealous, and that diplomacy would be expedient.

She then suddenly queried, "Who is Ferguson, light, pretty hands, dresses well, clean: he seems to have some connection with this question." She looked ahead some eight months to the latter part of August (1914) and saw the experimenter in outing flannels with a steamer rug; saw the steamer; said he was going on a mission to Greece, or Egypt perhaps; that there was a party being made up,—quite a number; that he was not going alone; that it was not advisable for him to be much alone; he gives out too much magnetism; he needs people about him, that he may enjoy their magnetism. She was emphatic in her assertion that it does not make any difference to her seeing the card whether the experimenter has it in his mind in any way, and that telepathy is a fact between people, between people and spirits, and between spirits.

The psychic has been holding circles for about seven years.

Reagent 5 gave her judgments from visual and auditory impressions. During the visual impression the card is shown to her by her "forces," sometimes in bright and sometimes in dark background. For the auditory impression a "neuter" voice, like intuition, speaks within her head. When a card is but dimly seen the voice names it; often after the voice has named a card it is shown her for visual verification. Sometimes three or four cards are shown at once and upon her asking "Which one?" one is either shown or named. The auditory impression is preferred, for that is the form in which her inspirational lectures come. There were no pure guesses. Confidence that the judgments were right was variable, but was higher, as a whole, on the second and third days' work (experiments 31-100). A very complete report was given of the form of imagery of the impression, permitting tabulation, and since the auditory form was preferred we may determine whether the preferred impressions (46 cases) are correlated more than the others with R cases:

	Card	Color	No.	Suit
Auditory, Per Cent	2.4	54	4.3	41
Probable, " "	2.5	50	10	25
Deviation	−0.1	+4	−5.7	+16

The deviation on Suit alone is large, and over half of it comes from R cases on "Cards Not Imaged"; if it were considered significant, its support for telepathy would amount to +13, and for clairvoyance or lucidity +20. But in view of the limit of chance deviation, for so short a series, being much larger (+27), and of the moderate deviation of the per cent of R cases for the complete set (+10, with limit of chance = +18.4),[214]

[214] *Vide,* Table XLI, set of 100, p. 89, *supra.*

no positive identification of a cause beyond chance can be made; perhaps a case for further experiment may be admitted, especially since the introspection on one of the cards completely hit (9:8) was "Beautiful light around it; thought I was looking; first with light; ray seemed like a search-light" (the card had been held in kinaesthetic imagery by the experimenter), although the judgment on the only other card completely hit was delivered before signal, while the experimenter was still shuffling the pack and had not yet drawn the card.

Other "messages" besides judgments of cards came: at the end of the second day's work we were informed that between us (the experimenter and his recorder), with a hand on the shoulder of each, stood Andrew Jackson Davis who wished to speak to us. He said, "Boys, this is not the highest way; get into the soul of things; greater things than this shall ye do. Both of you are in the soul of things more than you understand. This is the A, B, C you are working on now. I see it is very good; but not to your souls. You are on a higher rung than this will ever take you." The psychic said that the force was so strong about her that she could not get back to the cards, and continued: "He wants you to take up the higher principles. You must go right into the center—it's the very spring of life. The Mighty Highest gave different principles in a different way. I will give you some of the principles of life and tell you how God works. He works through perfect law and order and you got to come into perfect law and order to get the manifestations you are seeking. Just as the artist must have his tools so must every soul; it must come into possession of the principles of life before you can get what you seek. These things—every problem in the universe—can be demonstrated just as surely as a mathematical problem. You must get into this through your different powers. The principles of mathematics will work out every problem in the universe. Fire, air, earth, and water are the fundamental principles; seven notes of music, seven colors, etc. Everything must commence with the inner and then work to the outer. We got to learn the which and the what before we can handle these forces. Then we can do greater things than we have ever done. The room is full of spirits, very learned, very high order." [215]

One of the professional activities of the psychic is "psychometrizing," (she is an enthusiastic follower of Joseph Rodes Buchanan), and after the set of one hundred experiments had been completed she asked

[215] From the experimenter's shorthand notes, essentially verbatim.

that she might try an experiment so modified as to accommodate it to the conditions of this psychic activity. Consequently a card was drawn at random from the pack and, without its being known to anyone, placed in the card-box. After holding the box in her hand for a few minutes, and touching with her fingers the card within (her eyes being closed), she "sensed" the Five of Spades. It was the Nine of Hearts, as she immediately afterward learned. She said that one can be deceived by spirits, that he could not rely upon their impressions until he has learned by experience that they are true.

This psychic is sincere, and believes that the impressions are "given," but she is naturally somewhat uneasy because of the possibility of accurate check upon her impressions. This process, however, is essential, if the pertinence or force is to be removed from the agnostic's question: "Has she probably acquired confidence because of a few lucky hits, being shielded by the nature of many of the impressions which precludes accurate check, or the absence of a systematic check of such impressions as would permit it?" She has had automatic writing, but now confines herself to "psychometry" and the delivery of inspirational lectures on the Laws of Life. She has been actively engaged in psychic work for forty years.

Reagent 10_1, a veteran in the psychic world, took ten experiments in which she made six pure guesses, and three judgments, two from an impulse to speak (1:3,9) and one from an "impression" (1:8); after two respective judgments (1:8,9) she remarked, "No, I'm wrong." The only judgment not repudiated was right in color only. She had other impressions: a queer feeling in her back and the back of her head came from that pack of cards; she saw an old lady, very sick, "belonging" to the experimenter; also a canopy, which symbolizes death; and at 11:47 A.M. she saw a telegram on its way to the experimenter; it had reference to the sick woman. She saw other relatives of the experimenter who were in spirit life and had been attracted to the laboratory, and she gave their names: Mary A., D——, Carrie, Mary, John, Mary Elizabeth who had been called in earth-life Betsy, etc. The pure guesses on the cards were superior in R cases, although below the probable number, to the other impressions, for the latter were complete misses.

Reagent 10_2, a psychic of long experience, but now confining herself chiefly to inspirational lecturing, took thirty experiments. Her impressions came in visual and auditory imagery and "impressions."

There were only three guesses, but they were as successful as the judgments; there were no complete hits.

Reagent 10_3, an automatic writer, took ten experiments. His impressions came in kinaesthetic and visual imagery. His best judgment (1:5) was right in color and suit. The only complete hit was a guess. He had other impressions: discussed at some length a page missing from a book and three people supposed to be involved in its disappearance or in the experimenter's suspicion.

Reagent 10_4, a professional psychic for four years, took ten experiments, and all were pure guesses. She had much visual imagery, however: sees a beautiful star; a dark cloud underneath (1:1); a beautiful golden cross, white light underneath (1:2); a carriage (1:4); blue light and white light; red picture with gold in it (1:5); purple light, beautiful, with stars (1:6); boat on water; all men dressed in white, teachers of high order, coming towards experimenter; have lanterns in hands (1:7); two ladies on a golden road; one with a beautiful light about her head, pointing her finger toward the experimenter in whom she seems interested (1:10).

Reagent 10_5, a psychic of three years' experience, took twenty-three experiments. Her impressions came in auditory or visual imagery, and she gave six judgments the highest grade for certainty; two were complete misses, three had suits right, and one the number. She gave ten second grades, one of these being the only complete hit.

Reagent 10_6, a psychic of a few years' experience and now very active, took the remaining 17 experiments of the tenth set. Her impressions came in auditory form in her right ear, recognized as the voice of her control, "E——." The judgments in all characteristics of the card were much below the probable number.

It must be remarked that these experiments differ considerably from any of the practices to which the professional psychics are accustomed, and that, as already stated, they acted as reagents in their alleged normal state. Several of them have stated that they would have to "sit for development" for this particular sort of work before they could do it perfectly. In appraising the results, therefore, these imperfections of the investigation should receive consideration; but they should not exclude other considerations which would seem sufficient to qualify the present series of experiments as a fair preliminary survey of mediumistic capacity in psychical, as contrasted with physical, phenomena. The psychics, soon after a sitting began, uniformly entered an hypnoidal, semi-

trance state, quite comparable, so far as the experimenter could judge, to the state in which they do much of their professional work; they often recognized impressions as coming from their "forces" or "controls" and were almost uniformly expectant of statistical justification for their faith in the impressions, although they made no distinction between the "Card Not Imaged" and the "Card Imaged" conditions of the experiment; on account of the novelty of the work they did not expect all of their impressions to be made with sufficient skill to be perfect throughout. It would seem that the conditions of experiment were excellent, in spite of the novelty of the material, for engaging that capacity which reports in "messages" the location of a lost article, the richness of the ore in a distant prospect, or the state of the sitter's liver, since here the report was made on the color, number, and form of the spots on a card, the face of which was always exposed to the free air, either in the view of the experimenter or under the inverted pack held in his hand. The sittings occurred on three or four days, a week apart, amid surroundings similar to those in which the psychics conducted their customary work. If telepathic or clairvoyant processes occur in the psychic's activities, in a state short of the deep trance, or of a moment of critical experience (suspected by James [216] to be the essential condition), recognizable traces of it should have been found, it seems reasonable to infer, in our results.

The Private Psychics.

Reagent *6* received impressions in both auditory and visual forms; the auditory form consisted of a feminine vocalized voice speaking close behind the head and naming the card at once, as, "Eight of Diamonds"; the visual form presented the card, usually only the upper left corner of it, about reading distance from the face, as, the number "8", then a spot of Diamonds. The judgments were graded as to certainty, and there were but seven guesses in the set:

	B	*C*	*PG*
Number.....	80	13	7

Per Cent of R Cases in *B* Judgments.

	Card	Color	No.	Suit
	3.8	41.3	7.5	23.8
Deviation	+1.3	−8.7	−2.5	−1.2

The highly graded judgment occurs indifferently in the "Card Not

[216] *Proceedings S. P. R.*, 1896-7, 12:4. *Vide,* quotation in footnote 196, p. 124, *supra.*

Imaged" and the "Card Imaged" experiments, and it is not more reliable than the others. No relation is found between the mode of imagery and either the form of experiment or R cases.

The reagent has sat much in circles for development, has had automatic writing and psychometry.

Reagent *7* received impressions in three forms: the auditory is a voice more delicate than a whisper, located at the tympanum of the ear, is very distinct, positive, and certain; kinæsthetic impulse to speak, perhaps under "Indian control"; visual imagery of card in natural size, bright color, without distinct edges, located about reading distance from the face, on white background. The impressions are bare perceptions, having no meaning in card evaluation. Usually the imagery was in complementary form, not coördinated: as, visual for color, auditory for number, kinæsthetic for suit. The condition of the psychic during experiment is a semi-trance, the controlling influence repeatedly manifesting itself in automatisms of hands and body. Certainty and vividness of impression are pretty constantly correlated, and since 78 of the judgments were given the highest grade the deviation of per cent of R cases from the probable per cent for the set [217] is sufficient to show no relation between certainty and R cases. The three complete hits occurred in "Card Not Imaged" experiments. Two impressions were especially remarked because of special conditions (7:10 and 10:5); the judgments were both right in color, one in suit.

The reagent has sat much in circles for development and recognizes control; is reputed to have healing power.

Reagent *8* was a student who has had remarkable psychic experiences; he is intelligent, mature, a man of affairs, and a firm believer in occult phenomena. He is naturally introspective, and was able to give a very good account of his mental experience during the experiments. Most of his imagery was visual (35 cases); it was located usually less than a meter (sometimes as much as five meters) straight ahead, upon an uneven, foggy, distant background, sometimes luminous; it begins to show at about mid-interval and develops fairly quickly; if effort is necessary for its development, certainy of its value is lowered; its "flashing out" is more certain. His auditory imagery (4 cases) was not in a recognized voice, and had musical quality. Apart from some kinæsthetic impressions (8 cases) the rest of the cases were devoid of impressions. Certainty of judgment was dependent upon visual impression, vividness, distinctness, quickness, and persistence of imagery, sup-

[217] *Cf.*, Table XIV, (*B*), Reagent *7*, p. 63, *supra*.

plemented by a "hunch" which cannot be defined; it is merely a feeling of certainty.

The grading of certainty was definite:

	B	*C*	*D*	*PG*
Cases	6	11	31	52

The six highly graded guesses occurred in both "Card Not Imaged" and "Card Imaged" experiments, and fell below the probable number in R cases. Although the reagent did not consider the experimental conditions best for searching for occult phenomena, he candidly, on the basis of his experience in this set, expected an excess over chance in R cases, as is shown by the following deviations from the probable per cent:

	Card	Color	No.	Suit
Expected	+2.50	+20.	+25.	+20.
Actual	+2.50	+3.	+2.	+7.
Limit of chance *.....	+6.63	+21.	+13.	+18.

Of the five complete hits, four occurred in "Card Imaged" experiments; two were given the lowest grade and three were guesses.

Upon one occasion (6:5) there was a distinct visual image of a die, six spots up, two on the nearer side; the two-spot had been cast. Other imagery frequently occurred: outline of a spot of clubs, about three times the size of a card (2:5); a series of regularly arranged cards moving slowly downward (3:2); a cardium (fossil shell) (1:8); and the complaint was recorded: "other visual images interfering" (9:7, 8).

Reagent *9* was the same "sensitive" as Reagent *8*, but he worked under slightly different conditions. His earlier set of 100 experiments followed accurately the procedure observed by the normal reagents, even to the recording of the introspections. In this set he hoped to be able to profit by experience from experiment to experiment by checking judgment with card immediately after the judgment and his introspection on it were written. This change of procedure was not tried in any other series, for the reason that the mental attitude of the reagent would certainly pass through changes as fortune was seen to fluctuate. The wealth of imagery, and the introspective powers, of this reagent, made it seem likely that, in case thought-transference occasionally occurs, he would be able to identify its subjective attributes and thus revise the scale of evaluation for certainty. Even if he should fail to increase R cases to a large number, he might, at least, heap them up on his certain

* From Table XLI, set of 100, p. 89, *supra*.

judgments and thus wrest proof from an apparently chance series. Consequently, after the recording of judgment and introspection in each experiment, the experimenter announced the name of the card drawn and whether it had been a "Card Imaged" or a "Card Not Imaged" experiment. The experimenter was a post-graduate student of Philosophy and Mathematics, had been successful in parlor willing-games, both as guide and subject, and was judged capable of great concentration of mind. He was also a close friend of the reagent's, had acted as experimenter with the same reagent in the preceding set, and helped to work out the detail of procedure of this set, hoping to make it a crucial test. It was agreed that the reagent should choose such times for the experiments as he deemed most favorable. One minute was allowed for the interval of impression, which made the work arduous for both experimenter and reagent. The experiments ran from February 12 to May 9, 1914, and were performed on eleven separate days; the number of experiments performed on the consecutive days of experiment being 3, 7, 6, 7, 6, 8, 12, 6, 10, 19, 14.

The knowledge of wrong cases established an expectancy for continued wrong cases, and, as a whole, the grades fell lower than they were in the preceding set, although there were not so many pure guesses:

	A	*B*	*C*	*D*	*PG*
Number	0	1	7	52	40

If we inspect the eight higher grades we find them divided evenly between the "Card Not Imaged" and the "Card Imaged" experiments, and below the probable number in R cases, only one color and one number being right; no suit right. There were no complete hits in the whole set.

The introspective material contributed was copious and excellent; samples follow:

Auditory Impressions:

> Words heard soundlessly; as though spoken by another, but not heard. No kinæsthetic imagery of the vocal organs. Heard but once (color only right). (1:3).
>
> Heard my own voice; slight impulse to form words with tongue (a miss). (4:10).

Visual Impressions:

> Unusually persistent; some confusion as to number; at one time two hearts being uncertainly visible, one above the other (a complete miss). (1:4).
>
> Number symbols mostly; 3, then 4, then 6, then 3 persisting. Uncertain as to suit (color right). (1:6).

Confused number of symbols, hard to decide which; first, a red spot, then a diamond, then "V" unmistakable, but fading to dull spot from which, next, radiated four lines as to corners of card; decided, therefore, on 5 (wrong). (2:1).

No sense of color; form of diamond; figure 3 later (right for suit). (2:4).

Geometric figure, a hexagon, more distinct, lines in exceedingly dark red, almost black; heart inscribed, very faint, in black outline; heart not as persistent as outline; points in black (suit right). (3:7).

A heart, red, no number, fading quickly, then a more persistent geometrical outline, diamond-shaped, red (pale) lines in black (color wrong). (4:9).

6; then a heart, point up, quite strong (both wrong). (7:4).

Five geometric figures in succession, evolution of a spade (wrong). (10:4).

A heart broken in two pieces; (judged 2 of hearts, wrong). (10:10).

Kinæsthetic Impressions:

Unmistakably kinæsthetic—therefore astonishing (a miss). (2:5).

Impulse to say "four"; four what? "Why, *x*, of course!" Hunch said hearts (a miss). (9:7).

"Hunches":

Hunch said 7 (wrong). (6:4).

Hunch said Clubs; clubs became visible (wrong). (7:2).

Hunch said Ace of Clubs (wrong). (8:9).

"Impression":

Words distinctly formed mentally, as though spoken; not actually auditory; no decided kinæsthetic impulse; heard inside as though I spoke them (number right). (7:7).

Automatic Writing:

Tracing of part of outline, unmistakably spades (right). (10:2).

There was other imagery, as with the professional psychics, which did not relate to the particular experiment:

Extraneous Visual Impressions:

Words written on a black-board: "blue," "paste," "book," "Bess"—quite disconnected. (1:10).

Two hands, two wrists, small, woman's; but strong, tearing a deck of cards into small bits, very vigorously, and throwing them away. (4:7).

Bunch of unrelated geometric and astrological symbols. (5:10).

"Ru—dolph; Rudolfo; Rou—en; ———cy." (7:8).

Two button-hooks trying to hook into each other; also, on black-board, "Hogs." (10:3).

Three silver comets. (10:6).

"J19" on green maple leaf. (10:7).

Procedure "with knowledge" induced early in the set an inhibition of imagery: "Find I must vigorously guard against visualizing the first vagrant suggestion; and I find this decidedly inhibitive." (1:9). The interval of the experiment had been "Imageless, strikingly so! (Unusual in my experience!)."

The experience of the experiments justifies our regular method. The reagent cannot avoid vexation upon learning of misses.

The results of this set make it pretty clear that in spite of the best that two most favorably attuned and experienced persons in psychic matters could do toward perfecting the conditions for eliciting the processes of thought-transference or of clairvoyance (both of which had been several times most signally experienced by the reagent), the statistical results are obstinately negative, and the successful demonstration of the objective existence of either or both of these super-normal capacities is likely to involve subtle difficulties.

Conclusion.

The statistical analysis of the results of 1000 experiments with psychics reveals no advantage for the psychics over normal reagents as claimants for the capacities of telepathy or clairvoyance.[218] Although some of the deviations, either because of their consistency [219] or because of their size,[220] seem at first glance to warrant further experiments in search for some cause beyond chance, they are matched by chance deviations, and they fall considerably below the limit of chance deviation.

The *inner experience* of awareness of the card (whether telepathic or clairvoyant need not now be distinguished) frequently occurred in our

[218] This conclusion is, so far, in agreement with the admittedly incomplete experiments with Mrs. Piper, conducted by Professor James, who has recorded: "No sign of thought-transference—as tested by card and diagram guessing—has been found in her [Mrs. Piper], either in the hypnotic [not trance] condition just described, or immediately after it; although her 'control' in the medium-trance has said that he would bring it about. So far as tried (only twice), no right guessing of cards in the medium-trance. No clear signs of thought-transference, as tested by the naming of cards, during the waking state." (*Proceedings S. P. R.*, 1889-90, 6:654.) In England, Phinuit, Mrs. Piper's control, was given a brief series of tests in telling how many fingers were being held up by a person in the same room, but concealed behind a screen. There were nine successes (in the first guesses) out of 29 experiments. Professor and Mrs. Sidgwick observed that "his success, however, was not very startling, though probably beyond what chance would produce." (*Ibid.*, p. 616).

[219] *Cf.*, pp. 127 and 130, *supra.*

[220] *Cf.*, p. 133, *supra.*

experiments, and for some of the mediums it often consisted of "messages" or "impressions" from their customary "spirit controls." There seems to be no essential difference between the psychics and the normal reagents in the elements or forms of experience which contribute certainty to the judgment,—the apparent independence of the impression from the mind's voluntary activity being the best mark of a "certain" judgment. With the psychic, however, freer rein is given these *sensory and motor automatisms*; impressions come with greater facility and in greater variety, more prone to transcend the requirements of the experiment. Yet the *inner experience* of super-normal perception is no more closely correlated either with any single element under control in the conduct of the experiment or with R cases. When the "impressions" fell into professional form, they were no more reliable than when they pertained to the cards: *e. g.*, there was no financial interest related to the course to be continued next year, nor could a dark man have been jealous of it; the mysterious Ferguson with the pretty hands has not been identified. The experimenter did not enjoy familiar association with a steamer-rug at any time during the following August or since, nor did he board a steamer upon a mission to the land of Homer or of the Pharaohs.[221] Is the same authority equally reliable in her assertion that telepathy between people, between spirits and people, and between spirits is a fact? The telegram on its way to the experimenter, bearing sad news of an aged lady, very sick, "belonging to" him, graciously refrained from materializing; the names of his relatives in spirit land who were attracted to the laboratory—each and all failed to touch any chord of memory.[222] The absence of a missing page from a book still remains undiscovered and the three persons involved in either the misdemeanor or the experimenter's suspicion have been neither apprehended nor identified.[222a]

Without wishing to minimize the value of further experimentation with psychics in the "normal" state (for an accumulation of more data is desirable both for testing for super-normal perception and for psychological study of the psychic's mental processes) the writer is inclined to regard trance phenomena or hypnotic phenomena brought into relation to this investigation as the next logical step.

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[221] *Cf., supra,* pp. 132 f.

[222] *Cf., supra,* p. 135.

[222a] *Cf., supra,* p. 136.

III. "THE FEELING OF BEING STARED AT."

"The Feeling of Being Stared At" implies a telepathic process— a becoming aware in a super-normal way of a specific voluntary action of another person—and the results of its investigation may properly be included in Part I.

The belief in this phenomenon is probably an old one; and that it is shared by modern university students who come from all over this country, and from other countries as well, will presently be shown. Professor Simon Newcomb, the first president of the old American Society for Psychical Research, has said,[223] in an article which sums up his experience of fifty years in relation to psychical research, that from childhood be believed in telepathy through emotion, and in the power of causing any one sitting at a distance in front of one to turn and look around.

So far as the writer knows, no systematic experimentation has been undertaken, in psychical research, with a view of testing either "The Feeling of Being Stared At" or the power to cause a person to turn and look around. A few cases of the power to impress a percipient with one's personal presence, by willing it, however, have been recorded.[224]

The investigation recorded below was made for the purpose of testing the "feeling," and utilizing at the same time whatever "willing-power" the experimenter (agent) could exercise.

The Prevalence of Belief among University Students.

For five years the students in two of the larger classes in psychology (Mental Hygiene and General Psychology) have handed in replies to a Questionnaire in which the first and seventh questions were:

1. Do you ever feel that you are being stared at, with the conviction that your feeling can be relied upon?

7. Have you found that you could cause a person sitting in front of you in an audience to turn around, by "willing" it?

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[223] Newcomb: Modern occultism. *Nineteenth Century and After,* January 1909, 65: 127.

[224] *Cf., Journal S. P. R.,* 1887-88, 3: 307-8; 1889-90, 4: 323-6.

The following table gives the results: the class, the year of the class, the number of men and women respectively who handed in replies, and the per cent of them who answered both questions respectively in the affirmative.

TABLE LIV.

Per Cent of Affirmative Replies to Questionnaire Questions *1* and *7*.

Year	Sex	Mental Hygiene. No.	% "Yes" *1*	% "Yes" *7*	General Psychology. No.	% "Yes" *1*	% "Yes" *7*
1912–13	Men	51	86	55	68	60	44
	Women	44	86	88	78	74	40
1913–14	Men	65	80	68	61	80	50
	Women	55	96	85	77	91	80
1914–15	Men	62	69	53	86	49	26
	Women	62	89	82	40	48	33
1915–16	Men	87	77	56	108	65	57
	Women	44	89	59	71	80	66
1916–17	Men	27	78	44	122	83	62
	Women	23	87	57	55	89	82
Totals	Men	292	80	58	445	68	50
	Women	228	90	77	321	79	62
Men and Women		520	83	66	766	73	55
Grand Totals	Men				737	72	53
	Women				549	84	68
	Men and Women				1286	77	59

As a whole, 77% of 1286 young men and women, who have come from all corners of the earth and who were following subjects in all departments in the University, affirmed that they have experienced "The Feeling of Being Stared At" and that they regarded the feeling as more or less reliable. Many of these students related instances of proof, such as catching the starer, or hearing his performance commented upon by friends; and from the beginning of the questionnaire investigation some of the adherents to the belief, usually strong partisans of the hypothesis of "Mental Telepathy," expressed their willingness to put the matter to the test.[225]

[225] The belief in this super-normal capacity was found by Professor Titchener to be common among his junior students in Psychology at Cornell some years ago,

I. THE REAGENT REQUIRED TO JUDGE "YES" OR "NO."

During the year 1912-13, 1000 experiments [226] were performed by an aggregate of 10 reagents, each of whom made 100 judgments.

The reagents, consisting of one man and nine women, were selected, from the Class in General Psychology, on account of their faith in the belief. The five students who acted as experimenters for Sets *VI-X* were chosen from the same class because of their affirmative replies to both questions *1* and *7* in the Questionnaire. (The writer acted as experimenter in Sets *I-V*.)

During the critical interval the reagent sat in a writing-leaf chair with her eyes closed and shaded with her right hand, her elbow resting on the leaf of the chair. After the interval she gave a judgment "yes" or "no" according to her belief as to whether she had been stared at; recorded introspections indicating whether the state of her mind was favorable, and describing the content of consciousness upon which she based her judgment; and recorded a grade of certainty for her judgment.

The experimenter sat behind her, manipulated a dice-box by which he governed the nature of the experiment, tapped once with his pencil to signal the beginning of the critical interval, stared hard at the back of the reagent's head, neck, and shoulders if an odd number had been cast by the die, or closed his eyes and mused upon a favorite landscape if an

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and he reported in *Science* (1898, 8:895-7) that experiments in the laboratory proved the belief to be groundless.

Belief in the efficacy of "staring" has been remarked by others; *e. g.*, Hiram M. Stanley says, "Some say that they are able to make one sitting in front turn the head . . . by a steady fixed gaze," and he quoted from p. 198 of Mr. Bell's "Tangweera": "'Presently I felt as if someone was looking at me, and, raising my head, saw a large puma standing ten yards off.'" He regarded the belief as a manifest absurdity, but, considering that investigation has a practical value in exploding a common error, he put it to the test by a different method from that described in these pages: "I asked a young man who is very confident of his powers, to stand, unknown to reagent *A*, behind a book-case, and look through a carefully concealed peep-hole. I gave him the best opportunity, placing *A* about four feet from the hole and directly facing him, and I engaged *A* in mechanical writing. To the young man's confessed disgust and irritation he was unable to disturb *A*. My few experiments were negative in results." (Power of the eye. *Science*, 1900, 12:73).

[226] A brief report of these experiments was published in the *American Journal of Psychology*, 1913, 24:570-5.

even number had been cast, tapped twice to signal the close of the critical interval, and recorded in a ruled form the die number and the reagent's judgment. The staring was concentrated and was accompanied by a set of "will" that the reagent should "feel" it. The critical interval was 20 seconds for the first five sets, and 15 seconds for the remaining sets. Experiments at the beginning of the sets usually occupied about ten minutes each, but after the procedure became familiar and a critical attitude was assured they succeeded each other at a higher rate, never faster than one a minute. From three to four days, a week apart, were usually required to complete a set.

Results.[227]

The following table gives the results for both groups of reagents:

TABLE LV.

Total and Right Cases in Staring Experiments.

Reagent	Not Staring		Staring		Total
	Total	Right	Total	Right	Right
I.	51	28	49	22	50
II.	45	25	55	26	51
III.	62	23	38	20	43
IV.	53	24	47	25	49
V.	51	28	49	27	55
Avg.	52.4	25.6	47.6	24.0	49.6
VI.	45	23	55	33	56
VII.	56	21	44	23	44
VIII.	48	25	52	26	51
IX.	55	26	45	24	50
X.	52	22	48	31	53
Avg.	51.2	23.4	48.8	27.4	50.8
Grand Avg. ...	51.8	24.5	48.2	25.7	50.2

Of the 1000 guesses, 50.2% were right, (PE., 1.78: MV., 3.10); 47.3% of the guesses when the experimenter was "Not Staring," and 53.3% of the guesses when he was "Staring," were right. The die-spots came even 51.8% of the 1000 throws, conditioning this per cent of "blank" or "control" experiments.

[227] This discussion of our results is quoted with but slight revision from the report in the *American Journal of Psychology* (*loc. cit.*).

Since six of the reagents guessed "yes" in excess of "no" (*III*, 18 times; *IV*, 8: *VI*, 10; *VII*, 16; *IX*, 6; and *X*, 22), while but three guessed "no" in excess of "yes" (*I*, 10; *II*, 8; *VIII*, 2), resulting in a general average of 6 "yes" guesses per 100 in excess of the "no" guesses, the excess of 53.3% Right guesses "when the experimenter stared" over the 47.3% of Right guesses "when the experimenter did not stare," is without significance; if half of the excess of "yes" guesses is deducted from the "Staring" experiments, the 53.3% is reduced to 50.2%. The total Right guesses for each reagent is the significant figure. The limits are 43-56 and deviate from probability about equally; this size of deviation could be expected by chance 322 times in 1000.

Considering that theoretical probability is 50%; that our result of 50.2% falls between it and the experimental probability found by Quetelet [228] in 5460 drawings from equal numbers of white and black balls (white balls 50.48%); and that an experimental series of our own (frequency of odd numbers on the dice) for the same number of experiments gives 51.8%; we may conclude that no cause besides chance has been found working toward Right cases.

There are other ways in which the results may be distributed to show that there is no conspicuous "bunching" of Right cases in any of the rubrics, and that therefore the consistency of mutual support adds to the certainty that there has been no influence beyond chance operative toward Right guesses.

In some of the experiments, the distance between the experimenter and reagent was varied for the purpose of finding the influence of distance upon any factor above chance that might be found to be working for Right guesses. The following table gives the gross averages and the per cents of Right guesses for the various distances in meters.

TABLE LVI.

First Group.

Distance in meters	*1*	*2*	*3*	*4.6*	*6*	*10*
Number of Guesses	80	140	80	60	50	80
Per Cent Right	46.3	49.3	55	45	54	51.3

Second Group.

Distance in meters	*2*	*4*	*6*
Number of Guesses	160	100	20
Per Cent Right	45	53	50

[228] Quetelet: Lettres sur la Théorie des Probabilités, p. 57.

But, as Venn says,[229] anything may happen in a chance series, and it may be charged that all the guesses given with a low degree of certainty (a feeling that the guess stands a small chance of being right), by a freak of chance, may have run greatly under the probability-figure for Right guesses, and may thus have counteracted in our final per cent for each reagent the influence of a force working for Right guesses to be found in those guesses given with a stronger feeling of certainty.

A tabulation of Right guesses under their correlated grade of certainty (recorded in the introspections), however, shows no significant advantage on the part of any reagent for his more certain over his less certain guesses. The following table shows a total of such values from reagents whose grading was definite.

TABLE LVII.

Guesses Given with Varying Grades of Certainty.

Grades	*A*	*B*	*C*	*D*	*"Pure Guess"*	*Total*
Number of Guesses	15	332	264	61	36	708
Right	10	166	129	33	22	360
Per Cent Right	67	50	48.8	54	61	50.8

It seems pretty clear that, if there is a capacity to be aware of being stared at, it is not, as Richet thought of telepathic phenomena, shared to a slight extent by normal persons, but must be confined, as James suspected,[230] to subjects whose sensibilities have been augmented beyond a "critical point" through hypnosis or other abnormal conditions.

[229] "There can be no doubt that, however unlikely an event may be, if . . . we keep on trying long enough, we shall meet with such an event at last. If we toss up a pair of dice a few times we shall get doublets; if we try longer with three we shall get triplets, and so on. However unusual the event may be, even were it sixes a thousand times running, it will come some time or other if we have only patience and vitality enough. Now, apply this result to the letters of the alphabet. Suppose that one letter at a time is drawn from a bag which contains them all, and is then replaced. If the letters were written down one after another as they occurred. it would commonly be expected that they would be found to make mere nonsense, and would never arrange themselves into the words of any language known to men. No more they would in general, but it is a commonly accepted result of the theory, and one which we may assume the reader to be ready to admit without further discussion, that, if the process were continued long enough, words making sense would appear; nay more, that any book we choose to mention,—Milton's 'Paradise Lost' or the plays of Shakespeare, for example,—would be produced in this way at last."—VENN: The Logic of Chance, 3d ed., London; Macmillan, 1888, pp. 352-3.

[230] *Proceedings S. P. R.*, 1896-7, 12:4; also *cf.*, footnote 196, on page 124, *supra*.

Our reagents who had more or less confidence in their ability did not under the favoring conditions of our experimentation prove their power. Their belief must be largely based upon those subjective factors which enabled them to deliver some guesses with a strong feeling of certainty, and partly perhaps upon undue consideration of cases in which they have "verified" their feeling by catching the starer.

Qualitative Results.

Introspections show in what manner guesses are determined, and reveal the factors of experience that contribute to the guesses a feeling of varying grades of certainty that the guess is right.

Certainty is contributed to the guess by (1) some attribute or content of the imagery, (2) kinaesthetic sensations or images, or (3) inferences from sound sensations resulting from the experimenter's manipulation of apparatus, etc., or from other subjective processes.

(*1*) When the content of visual imagery involved the attitude of the experimenter, it determined the guess according to whether the experimenter was looking straight ahead or looking away. When this imagery was vivid, or if it appeared with facility (liveliness) and at the beginning of the period, or was persistent or recurrent, the guess was given with a feeling of greater certainty. (Reagent *I* said, "When the direction of the look is seen from the face only, I give the guess Grade *C*; if from the eyes, Grade *B* or *A*.")

The visual imagery may be weak, and when it appears at all be accepted as a sign of being stared at. For Reagent *II* visual imagery of the experimenter or of a school-room, in which she first experienced vividly the feeling of being stared at, yielded a "yes" guess.

Sometimes the content of the visual imagery was probably suggested by auditory impressions of the experimenter's movements when the latter were not pronounced enough to be singled out for "inferences" as treated in Section (*3*).

Those who depended largely upon visual imagery were Reagents *I*, *IV*, *V*, *VIII*, *IX*, and *X*.

(*2*) Some reagents were much occupied with kinaesthetic impressions during the interval. Thus for reagents *III*, *VII*, and *VIII*, the most characteristic cue for a highly graded guess was an almost irresistible impulse to turn around, or a tension of muscles in the neck and shoulders; for *X* it was a kinaesthetic (partly thermal) sensation in the right temple; sometimes the kinaesthetic impressions were not localized,

but were indicated by "a feeling of restlessness"; Reagent *VIII* also speaks of a "feeling of discomfort" with a "desire to turn."

The imagery of Reagent *V* involved a visual or at least a spatial element consisting of an imaged straight line, or beam, from the experimenter's eyes to the back of her head; and a marked kinaesthetic impression, leading to "yes" guesses given with a high degree of certainty, was tension of the eye-muscles toward this line. "Attention and eyes drawn toward line," was a frequent introspection for guesses given with a higher grade of certainty. She also has clear visual imagery of the experimenter either accompanying this kinaesthetic impression, and including the "line," or of the experimenter's face turned away. The visual element gave way, in the course of her experimentation, to the kinaesthetic, as a guide to the more certain guesses. Reagent *VII* also mentions this "line" in her visual imagery of the experimenter, and speaks of a "feeling of connection."

The kinaesthetic impressions involving restlessness, desire to turn, strain in the neck-muscles and in the eyes, were shared by other reagents who had other modes of imagery from which they made their guesses; as *V, VIII, IX.*

More subtle kinaesthetic imagery was sometimes evidently of influence in determining the guess. Reagent *IX* "felt like answering a call [of her name];" and *VIII* recorded a "feeling of being alone," which was a positive determinant for a "no" guess; and of a "feeling of being criticised," or a "feeling of nearness to the experimenter," both of which yielded "yes" guesses.

(*3*) Inferences were sometimes drawn by the reagent from sounds of the experimenter's manipulation of apparatus or his conduct of the experiment. After shaking the dice-box, the experimenter waited until the second-hand was coincident with a five-second dial-mark before he tapped. Reagent *I* noticed variability in the length of this interval, and inferred that longer intervals were caused by preparing "to stare"; and he confidently gave for these cases "yes" guesses; he sought for a basis for inference when at a point in his series impressions failed to come during the interval. Other reagents noted in the pre-period a sound of movement from the rustling of clothing, and inferred that the head was being raised "to stare"; when such impressions came within the interval, the reagent inferred that the experimenter was not looking. Reagent *VIII* "knew from her movements" the experimenter was not staring, and also inferred that "harder taps" were signals for a "yes" guess.

Even when such impressions are not used in "inferences" they may

conceivably influence the guessing by being taken advantage of subconsciously. It is impossible for the experimenter to maintain perfect uniformity in his conduct of the experiment, which involves, among other things, length of the various intervals, breathing, manipulation of the dice-box, intensity and accent in tapping, slight bodily movements, etc. Great effort was made, however, to maintain uniformity, and this may in part account for the lack of an excess of R judgments.

Inferences may also be based upon hypotheses, and depend in their outcome upon subjective conditions; *e. g.*, Reagent *VII* inferred from internal distraction that the experimenter was not staring, or the distraction would have been overcome; and entire absence of impression was inferred to indicate that none was sought to be made.

Other tendencies were also noted: "What did I answer last" influenced Reagent *II*, who was obviously endeavoring to keep positive and negative guesses about equally frequent. She also occasionally made up her mind, Marbe-fashion,[231] to say "yes" next time; but since the series was not voluntarily made by the experimenter, coincidence due to like tendencies of the two minds was excluded. And when she was "tired and bored" she wanted to say "no," as a general protest to further experimentation.

Conclusion.

Our conclusion, with respect to normal reagents, is (1) that the belief in "the feeling of being stared at" is quite common (shared by three-fourths of the university students); (2) that experiment shows it to be groundless; (3) that there is an explanation supplementary to that mentioned by Titchener [232] (nervousness, attracting attention, turning, catching the gazer) for the existence of the belief, lying chiefly in *attributing an objective validity to commonly experienced subjective impressions in the form of imagery, sensations, and impulses.* This is a tendency which, under favorable conditions, works itself out in Hallucinations and Motor Automatisms, and it seems to be a common trait in normal adults.

[231] *Cf.*, K. Marbe: Ueber das Gedankenlesen und die Gleichförmigkeit des psychischen Geschehens. *Zeitschr. f. Psychol.*, 1910, 56:241-263.

[232] *Op. cit.*

II. REAGENT REQUESTED TO RECORD HIS EXPERIENCE.

In spite of the fact that "The Feeling of Being Stared At" was experienced in the preceding series of experiments and that the feeling did not correlate with the staring, the results did not seem conclusive to some of the reagents who felt confident that they had proved their faith. The chief criticism ran somewhat as follows: "Your conditions in the laboratory are not identical with those in nature: specifically, the 'feeling' is normally experienced by an individual in an audience, or in the woods, and at a time when his mind is pleasantly passing from one thing to another until it is suddenly seized by the feeling. That is essentially different from being confronted with the question as to whether you are being stared at. Under the latter condition the reagent becomes self-conscious, suggestible, 'introspective,' and her experience must be abnormal and unreliable. Let her be stared at without her knowing it, and you'll find her 'feeling' reliable."

To meet this criticism a second series of 1000 experiments was performed, during the year 1913-14, under conditions more similar to nature, and, as is usual in compromising laboratory method with conditions of nature, obtaining results less amenable to statistical treatment.

As before, ten women, who had experienced the feeling, and expressed some degree of confidence in it, were selected from the class in General Psychology, to act as reagents; and ten women, seven of whom claimed that they had been able to "will" persons sitting in front of them in an audience to turn around, were selected from the same class, to act as experimenters.

The reagent was instructed to sit with her back to the experimenter (about 2 meters distant), to seek a calm, serene condition of mind during the interval of experiment, which would be from 15 to 20 seconds long, but yet to be on the alert for any impression which might seem to want to crowd itself into consciousness. She was not to seek any particular impression, was not to be searching or inquiring at all, but was to stand ready to welcome whatever should come. She was to record, after the interval, in phrases which she could explain later to the researcher, what ideas or impressions had come into her mind. She was told that the experimenter would shake a dice-box, record the number of spots cast, and signal the beginning of the interval of experiment with one tap of her pencil, and its close with two taps. She did not know it was to be a "staring" experiment, but was left with the impres-

sion that its principal aim lay in the completeness and accuracy of her recorded introspections.

The experimenter was instructed to shake the dice-box in a thorough manner, to keep the intervals between the parts of the experiments and between the experiments themselves as nearly constant as possible, not permitting the interval to run under 15 or over 20 seconds or the experiments to recur more rapidly than one a minute. She was to avoid moving the body during the interval of experiment, to stare hard, during the interval of experiment, at the back of the reagent's head, neck and shoulders, *and assume a determined attitude of will* that the reagent should "feel" the staring, when the die cast odd; but when it cast even, she was to keep her eyes and mind off the reagent, preferably by keeping the eyes closed and the mind upon a favorite picture, an ocean scene, a landscape, or something quite impersonal, during the whole interval. She was cautioned to keep her instructions to herself, owing to the effect upon her own reagent and upon the other reagents if gossip started. The time was observed from the second hand of a watch, or stop-watch, but was soon accurately estimated so that the watch merely acted as a check to the estimating.

Results.

In the ten series of 100 experiments each, the odds occurred 43, 44, 46, 49, 50, 52, 54, 54, 56, and 59 times, aggregating 507 times.

Distraction was noted 71 times, 40 during intervals conditioned by odd die-spots, which left 467 intervals of "staring" while the reagent considered mental conditions favorable.

Among the 1000 introspections there is not one which suggests that any of the 10 reagents had "the feeling of being stared at" during any of the critical intervals. About 20 out of the 1000 may be regarded as indicating a reaction upon the conditions set by the experimenter; such as

(1) Condition of mind good; image of experimenter sitting at table back of me; saw her hand holding a dice-box. (1:55; odd).

(2) Fair; distraction of radiator; saw experimenter at table; saw stray lock of her hair. (1:60; even).

(3) Good; saw two four-spots (of dice). (2:7; even—2).

(4) Good; saw a long row of dice. (2:90; even).

(5) Not good; wondered what was going on behind me. (3:1; odd).

(6) Good; rather wondered whether experimenter was suggesting any impression. (8:16; even).

Of the twenty, eight were written upon intervals conditioned by the odd die-spots.

Why was not "the feeling of being stared at" aroused? In the former series it occurred but it was not reliable; in this series the reagent's mind was relieved from self-consciousness, and the reputed cause of the "feeling" was introduced during 467 favorable intervals without result.

The cause of the "feeling," or, at least, the factors of experience upon which confidence in it rested, were found to be (1) some attribute or content of the imagery present, (2) kinaesthetic sensations or images, or (3) inferences from sensations of sound resulting from the experimenter's manipulation of the apparatus, etc.[233] And it was concluded that its cause lies "chiefly in attributing an objective validity to commonly experienced subjective impressions in the form of imagery, sensations and impulses."[234] This tendency seemed a common trait in normal adults.

An inspection of the introspections reveals the fact that plenty of subjective impressions, in the form of imagery, sensations, and impulses, were experienced in this series of experiments, and there is indication that some of them stand on the threshold of a veridical interpretation:— might be regarded as cases of incipient clairvoyant or other "psychic" capacity:

(1) Poor;[235] saw chair empty in next class. (1:9; odd).

(2) Good; saw girl with whom I am going to game; saw her on bleachers, expression on face, dress she will wear. (1:25; odd).

(3) Good; saw postman coming to door, mother on porch receiving letter. (1:57; odd).

(4) Good; seemed to be looking down stairs of this building; saw color and texture of stairs. (1:64; even).

(5) Good; saw grandmother who is 500 miles away. (1:79; even).

(6) Good; saw neighbor getting off train; she is expected home today. (1:98; odd).

(7) Good; thought I heard some one saying my name. (2:19; odd).

(8) Good; heard whisper "4" back of experimenter. (2:1; even—2).

(9) Fair; "ninety-three." (9:4; even).

(10) Good; thought of the number "three." (10:37; even).

(11) Good; saw hands of library clock ten minutes after four. (5:74; even).

(12) Fair; hand itches; am I going to meet some one? (7:30; odd).

(13) Good; felt as if I wanted to speak. (2:8; even).

(14) Good; impulse to put hand to head. (6:26; even).

(15) Good; felt as if trying to pronounce words. (10:33; even).

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[233] *Vide, supra,* pp. 150 f.

[234] *Vide, supra,* p. 152.

[235] Condition of the mind during the critical interval.

The reason that these impressions, sensations, and impulses do not have the attribute of objective validity seems to be that the situation was not recognized by the reagent as one in which veridical impressions are likely to occur. Let the suggestion be given that impressions received during the interval may not be entirely free from the experimenter's control, and the attribute of objectivity in varying degrees of certainty is conferred upon them, as was the case in the Card-guessing, and in the first series of Staring experiments.

Supplementary Experiments.

To test this further with Staring experiments, but under conditions in which the suggestion was not so direct, two more young women, who regarded themselves as being sensitive for staring, were selected from the class in General Psychology to act as reagents, and their experimenters were similarly chosen for their faith in their power to will persons to turn around. Each pair performed 100 experiments. Procedure was the same as in the main series, except that the reagent was given the following instructions:

"During the critical interval you are to keep your mind free from external and internal distraction, open for any impression or suggestion that wants to come into consciousness; this may not be entirely free from the experimenter's influence."

Introspections were recorded as before.

In the *first* supplementary set the die cast odd numbers, conditioning staring, fifty times, and there were twenty-six introspections the content of which referred to the experimenter's influence in "Willing," transmitting thought, and "Staring"; 58% of them occurred in experiments when the experimenter stared, and 42% when the experimenter did not stare. The following are samples:

2:10. Felt as if the experimenter's attention was upon me. (5).[236]

3:10. I thought the experimenter had some influence on thought; she was thinking about me. (2).

4:4. Thought the experimenter was looking at a certain spot on my head, low down; wanted to turn around and see. (3).

6:3. Felt she must be looking at or thinking about me. (6).

7:1. Felt something pulling me her way. (6).

7:5. Wanted to ask her if my hair was all right; felt her looking at it. (1).

8:7. She seemed to be looking at me. (4).

This reagent explained that she suspected, from the instructions given her, that the experimenter would attempt to influence her by

[236] Die-spot.

thinking or staring when certain die-spots were shown. Early in the set she noticed that the experimenter's tap signaling the beginning of the interval varied in strength, and inferred from a sharp tap that the experimenter would either think about her or stare at her. From this cue, which did not turn out to be very reliable, her experience during the interval readily developed.

In the *second* supplementary set the die came up odd 51 times, and 25 introspections were marked by the reagent as records of experience probably caused by the experimenter. They were classified into two groups according to the reference of the content to the experimenter's thoughts or to her actions; out of twelve of the former, five, and out of thirteen of the latter, seven, fell upon experiments when the experimenter stared. There were five cases of the feeling of being stared at, four of them falling on experiments conditioned by odd die-spots. The more pertinent of the marked introspections follow:

6:4. Experimenter's paper and pencil: her hands. (5).
6:8. Experimenter looking at me. (1).
7:10. Experimenter looking at dice. (4).
8:3. Experimenter looking at me and at paper. (6).
8:8. Experimenter watching me. (1).
9:1. Experimenter looking from me to paper. (1).
9:8. Experimenter looking at watch. (6).
9:9. Experimenter looking at me. (1).
10:5. Die with five spots on top. (6).
10:6. Experimenter looking at my back. (6).
10:8. Experimenter holding pencil. (1).

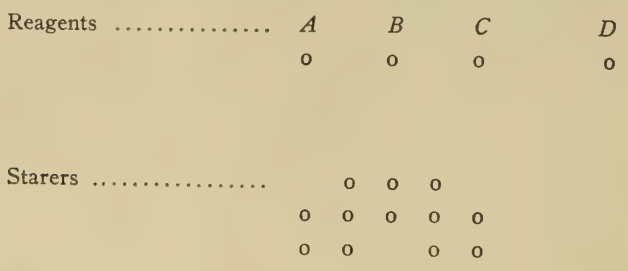
Hit and miss cases of the feeling of being stared at occur in a variety of impressions, some of which are fair samples of incipient clairvoyance and compare not unfavorably with those in which "psychics" place unreserved confidence. For these impressions to be pertinent, only slight suggestions in word or external condition appear to be necessary.

When the situation, however, is not recognized to be probably a "staring" one, the reagent, although his mind is free from the self-consciousness undoubtedly induced by the method of the first series and is in as favorable a state as could be desired for being seized with the feeling of being stared at, has impressions which occasionally approach "psychical phenomena," but which, as we have seen in the first part of this series, are devoid of any reference to frequent and competent staring.

III. MULTIPLE STARERS.

One more criticism seemed to be worthy of test. It was suggested that the force of a single person's staring might not be sufficient to rise above the threshold of awareness, under laboratory conditions, and it may be for this reason that laboratory results do not confirm the belief which has grown out of general experience.

A series of 100 experiments was consequently carried out during 1913-14 for testing this criticism. Four reagents were chosen in like manner as before for their special qualifications; Reagents *A*, *B*, and *C* (women), because of their sensitiveness for staring, and Reagent *D* (a man), because of some training in reading slight sensible signals. *Twelve starers* were appointed, five women and seven men, some of whom were somewhat skeptical, but all of whom were willing to do their best to give the "feeling" hypothesis a fair trial. Only on the last day, however, when series 8, 9, and 10 were performed, were all of the starers in their places; on the first day two men and on the second day three men were absent. The sittings took place in the large lecture room (No. 414) from 11:15 to 12:15 on Fridays (Nov. 6, 20, Dec. 4). The disposition of the reagents and starers is shown below, the distance between the reagents and the first row of starers being a little over two meters:



The instructions to the reagents *A*, *B*, and *C* were to rest comfortably with eyes closed, during the critical interval, and to keep the mind in the most favorable condition, according to their judgment, for determining immediately after the interval whether they had been stared at, for at this time they were to record their judgment in "Yes" or "No," and were to write the customary introspections descriptive of their experience. They were informed that the staring would be controlled *pari passu* by the dice-spots. Reagent *D* was instructed to keep

his eyes closed, during the interval, and to endeavor to judge as best he might from slight sounds such as are made by the movement of the body, of the feet, of clothing, or by changes in the breathing, whether the starers were staring.

The starers were instructed that when an odd number was cast by the die they were to *stare concentratedly at the second person from the left* (Reagent *B*) ; but when an even number was cast to close their eyes, during the critical interval, and image (intensely) a black cat on the lecture table, facing the left. In either case, during the interval they were to assume a strong attitude of will that the second person from the left should share their mental content. The starer who sat in the middle of the front row was appointed master of ceremonies. He shook the dice-box, held up the die so that all starers could see the spots cast, kept the time, and tapped signals to begin and to close the critical interval, which was 15 seconds in duration. At the beginning the experiments ran slowly; later they recurred at the rate of one a minute.

At the conclusion of the series of experiments the starers were asked to hand in a written statement describing their experience during the interval, telling exactly what they did when an odd number was cast, what they did when an even number was cast, and describing any deviation during the hour or between days of experiment.

Results.

Of the 100 throws, odds were cast 55 times; hence, in 55 experiments the crowd of starers stared at Reagent *B*, and in 45 experiments they closed their eyes and sought to transfer to Reagent *B* their respective images of a black cat.

Reagent *B*, who had been chosen as the most sensitive of the three, the only reagent who was stared at, recorded "Yes" 34 times; 56% were right; of the 100 judgments, 49% were right. The probable per cent is 50.

The multiplication of starers undoubtedly increased the vividness of the experience, as is shown by the reagent's introspections. Most of those accompanying her "Yes" judgments are given below in two lists according to whether the reagent had been stared at:

(1) Reagent Stared at.

1:5. Heart beat faster.
7:5. Heart beat faster; expectant feeling.
2:6. Felt warmer.
2:9. Thumping in ears (felt skeptical).
10:9. Felt conscious of my ears.

1:8. Felt choked; tightening of throat.
3:7. Queer catching of the breath.
4:3. Felt throbbing at back of neck; mind a blank.
3:4. Kinaesthetic sensations in shoulders.
4:8. Kinaesthetic sensations in shoulder.
5:9. Kinaesthetic sensations in arms and hands.
4:4. Hard to describe my feelings: just felt I was being stared at.
8:7. Felt "conscious."
8:2. Expectation and feeling of realization.
8:5. Felt several pairs of eyes on back of my head.
9:3. Felt eyes on the back of my head.
9:10. Conscious of presence of those behind me.
1:4. No definite feeling except "consciousness."

(2) Reagent Not Stared At.

1:10. Kinaesthetic sensations at back of neck and in cheeks.
2:10. Muscular movements in back.
2:1. Tightening of muscles inside; faster heart beat.
1:3. Felt like more blood rushed to my head.
2:3. Breathing hard; expectant feeling.
7:6. Breathless for a moment, reeling.
2:5. Queer feeling at pit of stomach.
6:4. Felt twitching of ears.
10:10. Felt conscious of my ears.
10:4. Kinaesthetic sensations in my right shoulder.
3:1. Indescribable feeling of self-consciousness.
5:5. Felt like I was being looked at; ears got hot.
10:6. Thought I felt their eyes; wasn't sure.
3:6. Mind concentrated on self.

An examination of these lists reveals (*a*) the sensitiveness of the reagent, some of her experiences being almost painful and some of them quite embarrassing; (*b*) the elements of her experience upon which the affirmative judgment is grounded; and (*c*) the occurrence of these elements impartially in both lists. Although the first list seems somewhat more satisfactory, the introspections being greater in number and some of them more positive in statement, the difference would hardly be satisfactory to any believer in the theory for use in supporting his belief.

The results of Reagents *A* and *C* fall in the same class, since neither of these reagents was stared at.

Reagent *A* judged "Yes" 14 times. Some of the introspections accompanying her affirmative judgments indicate the elements of experience responsible for them:

1:4. Strong feeling in center of back; tendency to turn around. (1).
1:7. I felt a tendency to turn to my right like some one over my right shoulder was thinking of us. (5).

2:1. Felt cold—shivered. (2).

3:3. Strong idea that it was cold. (1).

4:4. Felt impression in middle of back. (1).

4:7. Wondered if Reagent *B* was feeling what I was; could not tell whether I felt her being stared at. (4).

5:2. Thought how peculiar and foolish experiments were and wondered how they did any good; and yet felt I was being stared at. (6).

5:6. Felt as if the starers were staring at me; and the other reagents not entering (not being stared at). (5).

5:8. Rather strange feeling—very centered. (6).

6:8. Felt leader's eyes upon me. (1).

8:4. Felt people staring at my head, and also at Reagent *B*. (4).

The effect of the suggestion furnished by the setting of the experiment is also shown by several cases in which the experience was on the point of developing and again in which it had been begun but did not mature into affirmative judgments:

6:7. Thought it was about time, but felt nothing. (6).

7:4. About time, came over me, for some one to stare at me; no feeling, however. (6).

3:4. At first, no thoughts; later, thought of starers, but effect was gone. (3).

6:2. Felt a wave of something, like some one thought about staring but did not. (2).

Reagent *C* made 16 "Yes" judgments. Her introspections accompanying the latter indicate the positiveness of her experience:

1:3. Felt as though some one were staring at back of head. (4).

1:5. Felt self-conscious; had desire to laugh. (3).

1:9. Had a feeling of being stared at, and a sort of visual image of starer (eyes, etc.). (4).

2:6. Had a sort of dull feeling in head as though headache was coming on. (3).

3:1. Felt that they kept on staring after two taps. (2).

3:9. Felt I was being stared at in back of head. (1).

4:6. Felt as though starers on right side did most of the work. (2).

4:10. Felt great desire to turn around and look at [the person] who was staring at me. (5).

5:8. Quite sure; felt however as though they were not staring with all their might. (6).

5:9. Felt some one staring. (3).

7:7. Felt as though some one were staring at my back. (3).

8:1. Felt distinctly that I was being stared at. (6).

9:4. Felt that starer on left side was "doing the work." (5).

9:6. Had distinct idea of being stared at. (6).

10:2. Felt that I was being stared at. (4).

10:10. Felt this time that I was being stared at. (6).

Other introspections, accompanying "No" judgments, indicate incipient stages of the feeling of being stared at:

2:10. Was not quite sure; hesitated about saying "no." (2).
4:2. Slight doubt as to "No." (6).
4:7. Slight doubt as to "No." (4).
6:3. Was not positive enough to say "Yes." (6).
9:1. Was not quite sure about saying "No." (2).

The fruitfulness of the experimental situation for suggestion may be seen in still other introspections written by reagents *A*, *B*, and *C*; first, regarding others being stared at; then a few miscellaneous cases:

REAGENT *B*.

4:1. Felt as if they were staring at Reagent *C* at my right. (5).
5:8. Felt Reagent *A* was being stared at. (6).
7:2. No sensations: felt others were being stared at. (5).
9:8. Felt as though my neighbors were being stared at. (2).

REAGENT *A*.

10:10. Felt that the starers missed me and were staring at the chair next to me, not the person (Reagent *B*). (6).

REAGENT *C*.

2:3. Felt "No, they are staring at some one else." (4).
4:5. Felt as though reagents on my left were being stared at (Reagents *A* and *B*). (4).
7:3. Felt as though some one was staring, but not at me. (1).
9:7. Felt that other reagents were being stared at. (3).

MISCELLANEOUS CASES.

C. 3:7. Found myself saying inwardly "Is some one staring at me?" (1).
C. 6:7. Found myself acting as though I were starer, saying "I am looking at you"; peculiar condition. (6).
C. 6:8. Still wondering about preceding experiment. (1).
A. 3:8. Saw "No, no," on my paper. (1).

The statistical and administrative phases of the experimentation were not without suggestive power:

B. 7:10. Speculated on how many times I've been wrong.
B. 9:6. Speculated on outcome of this experimentation.
B. 10:10. Anxious to know outcome of experimentation.
A. 4:5. Wondered how they marked the person to be stared at.

Occasionally, as in the card-guessing, the impressions, although vivid and definite, had no bearing upon the matter in hand; as,

A. 7:7. Braced my mind to think on nothing, but suddenly a visual image of a street in Palo Alto and the Episcopal church came before my eyes.

The chief contribution of the introspections of this series, however, lies in their illustration of the fact that *if the situation is recognized by the reagent as probably a staring situation, "the feeling of being stared at" is quite likely to be evoked in grades of definiteness from doubtful certainty to painful and embarrassing certainty;* and that *the reagent's incentives for conferring the objective attribute upon his inner experience are derived from his acceptance of the suggestions of the situation, and not from the objective fact which his experience seems to him to definitely and truly report.*

If the judgments of Reagents *A* and *C* are regarded as referring to the staring at Reagent *B* instead of at themselves, Right cases are 45% and 43% respectively, as against the probability of 50%.

The R cases of Reagent *D*, whose judgments referred to the fact of staring only, are 50%. His introspective report is a vindication of the method of experiment, with reference to the oft-repeated criticism that slight sounds furnish the sub-conscious cue for the reagent's judgments. The imaging of the black cat on the demonstration table during the intervals of no staring was designed to assist the starers to keep the conditions of the regular and the control experiments uniform. Owing to the number of starers it was feared that such unconscious indications might be sufficient through their cumulation to introduce error. Since Reagent *D*, who judged "No" when he heard rustling of clothing indicative of movement or shifting of position, feet tapping or striking or scraping a chair or the floor, chairs moved, paper rattled, or sighing, and judged "Yes" when there was "no sound," "absolute silence," or "utter silence," was not able to raise his R judgments above mere chance, the field would have been clear for identifying a super-sensible cause in the R cases of the feeling of being stared at, had they not occurred impartially upon both regular and control experiments, and with the control reagents as well as with Reagent *B*.

Another vindication of our methods of experiment in the laboratory, using as reagents students selected from classes in General Psychology, should perhaps be pointed out while the conditions of this series of experiments are fresh in the reader's mind. The fact that none of the reagents saw a black cat on the demonstration table, indicates that a group of sixteen students have been sufficiently faithful to their trust that during the three weeks of experimentation no breath of gossip reached the ears of the reagents.

Of course, the non-appearance of the black cat also indicates (1) that a sensitive reagent can spend 45 periods of 15 seconds each in a state of mind judged by herself as most favorable for receiving a

feeling of being stared at, and be the unconscious target for shafts of visual imagery from twelve persons, without being aroused by the cumulated energy of thought-projection; and (2) that two reagents, more or less sensitive, can be in close proximity to and carelessly exposed 45 times to the same battery of black-cat images without danger of being hit.

Since none of the reagents got a "generic image" [237] of a cat "compounded from the minds" of the twelve starers, no description of it was available from the introspections, but the reader may be interested to learn some of the characteristics of the respective cats as imaged in the different minds. The following four quotations will be sufficient to illustrate the diversity in detail:

(1) A huge black cat sitting on the table, looking out of the window.

(2) I always thought of the same black cat in the same attitude. It was always the bushy-tailed, "stocking ad" cat with said tail elevated at an agle of 40°.

(3) I closed my eyes and thought of a black cat; it very often looked like the one in the shoestore back of the postoffice; its eyes would move, though, and its whole body sway, very often. . . . Sometimes it would be very quiet and still, and pop its eyes right out and stare with a glassy look.

(4) The cat had his back arched; his eyes were green; the hair on his neck stood upright; he was a fierce cat.

This diversity in detail following general instructions to image a black cat is not greater here than the writer has found it in a sitting where several professional 'psychics' saw clairvoyantly an ethereal being in the room and later independently described the detail of position, posture, features, raiment, etc.

The reports handed in at the close of the experimentation show that the starers knew their duty, and that there is every reason to believe that they acquitted themselves well. Most of them were favorably disposed toward the "feeling" hypothesis; some were agnostic. Sample quotations follow:

(1) Personally, I have always been somewhat attracted to the idea of Mental Telepathy. . . . There are times when one does feel like he is being stared at and finds it to be true, and there are cases when one is unaccountably depressed or elated and finds out later that some good news, or the contrary, was on the way, or that something pleasing or displeasing has happened; these incidents being such that he could not possibly have known anything about them. I suppose, of course, in anything of this kind we always neglect, or at least have a tendency to neglect, negative instances, and this will account for a good deal of the assurance with which some people declare their belief in Mental Telepathy. . . . It is undoubtedly true that when you are closely associated with some one who is feeling "out of sorts," although he carefully conceals the fact, you are affected by his depression.

[237] *Cf.*, Jordan: The sympsychograph. *Pop. Sci. Mo.*, 1896, 49: 597-602.

Why is it not possible for stronger feelings of a like nature to be transmitted greater distances? . . . I have always had a certain amount of belief in it.

(2) I am not skeptical regarding the experiment.

(3) As for the value of the experiment I do not see but that there should be some results worthy of note. I do believe in some form of Mental Telepathy.

(4) Although I have never experienced the feeling of being stared at, I think it is quite possible for one to have that feeling, for many times I have looked up to find some one staring at me and thought at the time that this caused me to look up, although the *definite feeling* was lacking.

(5) Since I have never had a conclusive proof of any positive effect of staring at another's back, I naturally have not much faith. However, I am hoping to have this point settled when the results of the effects on the reagents and their attitudes of mind are known.

(6) I am a skeptic on this subject . . . but this does not mean that I have certain preconceived ideas which are not possible of eradication. I am open-minded enough to be willing to consider the possibility of anything. But just one objection occurs to me in this connection; namely, that if this faculty is at all wide-spread its evidences should be so manifest as to require no discussion. A professor when he asks a question thinks of the answer he expects, but the student never flatters himself that he gets any enlightening suggestions from this method. . . . In a word the facts of everyday life all point away from such a conclusion. The scien-
stared at. . . . I have never been conscious that any one has been staring at me.
tific aspect of the subject I do not pretend to know anything about.

(7) I do not believe that any accurate results [of proof] can be obtained. I think that it is only a matter of chance in knowing [judging] whether one is being

How impotent the combined staring, and the combined effort at projecting the imagery of a black cat, proved to be, and how fortuitous was the occurrence of the feeling of being stared at, can be more fully realized, perhaps, by inspecting a few of the experiments in cross-section; thus getting a record of the synchronous experiences of the respective reagents under the two general conditions of experimentation:

REAGENT *B* WAS STARED AT.

1:4. *A*. Strong feeling in center of back; tendency to turn around.—Yes.[238]
B. No definite feeling, except "consciousness."—Yes.
C. Mind in normal condition.—No.
D. Heard nothing at all: silence.—Yes.

1:7. *A*. Felt a tendency to turn to my right, like some one over right shoulder was thinking of me.—Yes.
B. No sensations.—No.
C. Required conscious effort not to follow a train of thought.—No.
D. Heard no sound or movement.—Yes.

[238] To interpret the line: The numbers indicate the fourth experiment of the first series (of ten); the letter indicates the reagent; the judgment following the record of experience relates to the staring.

4:10. *A.* Closed eyes; tired feeling.—No.
B. No sensations.—No.
C. Felt great desire to turn around and look at [the person] who was staring at me.—Yes.
D. Heard nothing.—Yes.

7:2. *A.* Cold; shivered.—No.
B. No sensations; felt others were being looked at.—No.
C. Normal condition.—No.
D. Silence in the rear of room.—Yes.

A Black Cat Was Imaged, and the Thought Directed Toward Reagent *B*.

1:3. *A.* Wondered when the tap would come (to close the interval).—No.
B. Felt like more blood rushed to my head.—Yes.
C. Felt as though some one were staring at back of head.—Yes.
D. Heard a shifting behind me.—No.

2:3. *A.* Wondered how high the flag-pole was.—No.
B. Breathing hard; expectant feeling.—Yes.
C. Felt "No, they are staring at some one else."—No.
D. Complete silence behind me.—Yes.

2:10. *A.* Wondered how many experiments. Whether they really were staring, and I could not recognize it.—No.
B. Muscular movements in back.—Yes.
C. Was not quite sure: hesitated about saying "No."—No.
D. Heard nothing.—Yes.

4:7. *A.* Wondered if Reagent *B* was feeling what I was. Could not tell whether I felt her being stared at. —Yes.
B. Thought of all the things I should be doing at home.—No.
C. Slight doubt as to "No."—No.
D. Heard no sound behind me.—Yes.

5:8. *A.* Rather strange feeling; very centered.—Yes.
B. Felt Reagent *A* was being stared at.—No.
C. Quite sure; felt, however, as though they were not staring with all their might.—Yes.
D. Heard no sounds.—Yes.

6:7. *A.* Thought it was about time, but felt nothing.—No.
B. Had auditory image of sounds in Psychol. yesterday.—No.
C. Found myself acting as though I were starer, saying, "I am looking at you"; peculiar condition.—No.
D. Heard a sound of shifting of position.—No.

9:1. *A.* Wondered how long this series would take.—No.
B. No unusual sensations; thoughts wandered.—No.
C. Was not quite sure about saying "No."—No.
D. Complete silence at rear of room.—Yes.

Conclusion.

"The Feeling of Being Stared At" is experienced in the laboratory when the experimental situation is recognized by the reagent as probably a staring situation. It does not correlate with the external fact of staring whether the reagent is or is not stared at during the experimentation, or whether the staring is done by one person or by twelve persons. When the situation is not recognized by the reagent as providing staring, the feeling does not occur. It seems probable, therefore, that the occurrence of the feeling is dependent upon suggestion alone, having no causal relation to the fact of staring.

Analysis of experience reveals imagery, sensations, and impulses as the *points de repère,* if not the essential constitution, of the "feeling," although many attempts in the introspections left the "feeling" unanalyzable. Certainty often depends upon the vividness of the imagery and its apparently independent behavior.

In all situations where the reagent seeks to free her mind from all distraction, external and internal, impressions in great variety insinuate themselves into consciousness; some of these have no apparent relation to the business in hand. In this respect normal reagents and sensitive reagents are similar. While the former, however, confer objectivity only upon those impressions relevant to the experiment, the latter regard their irrelevant impressions as veridical either in their literal or in a symbolic sense. It is true that the psychic is likely to enjoy impressions in greater profusion; but this may be owing to her practice as an automatic instrument for the manifestation of what she regards as other (usually 'spirit') personalities or for the expression of as yet unknown cosmic forces.

Sensory and motor automatisms, then, from incipient to hallucinatory grade, are shared by both normal and sensitive reagents, and constitute the *inner experience* of "the feeling of being stared at" and of thought-transference, but, so far as our experiments (which number in the aggregate 14,500) are qualified to indicate, bear no causal relation to the external processes or facts to which they refer.

PART II.

EXPERIMENTS
ON
SUBLIMINAL IMPRESSION

May there not be around our normal perception, a fringe of perceptions, most often unconscious, but all ready to enter into consciousness, and in fact entering in in certain exceptional cases or in certain predisposed subjects? If there are perceptions of this kind, it is not only psychology in the strict meaning of the term that they concern; they are facts with which "psychical research" could and should concern itself.—PROFESSOR HENRI BERGSON, in his *Presidential Address,* delivered in Aeolian Hall, London, May 28, 1913, before the Society for Psychical Research. (*Proceedings S. P. R.,* 27: 170.)

"The most interesting and illuminating address which this Society has ever received."—ARTHUR J. BALFOUR. (*Journal S. P. R.,* 16:86).

PART II.

SUBLIMINAL IMPRESSION.

ORIENTATION.

Apart from deception, collusion,[1] a disregard for the limits of chance,[2] and "brain-waves," [3] the explanation for thought-transference most frequently brought forward, especially by scientists who have some acquaintance with psychology, is that of subliminal perception of signs or signals involuntarily given. It is supposed that the reagent, or percipient, unconsciously receives impressions in the form of signals or signs offered involuntarily by the experimenter, or agent, or some party or parties to the experiment who know what the reagent is attempting to guess, and that these signs or signals may be so slight that an acute observer would not perceive them and might be willing to affirm their absence. Thus, in the investigation of the famous "educated horse," Clever Hans, Pfungst found that this horse, popularly credited with an education equivalent to that of a seventh- or eighth-grade boy, could paw the answers to problems in higher mathematics, problems beyond the range of many American college graduates, provided only his fine old master von Osten knew the answer and was in his field of vision.[4] The pawing stopped at involuntary signals, consisting of slight changes in posture and expression, so slight that no observers had noticed them and some are yet skeptical of their existence. And, concerning the re-

[1] Tuckett: The Evidence for the Supernatural. London: Paul, 1911, Appendix R, pp. 369-399; also Gurney: *Proceedings, S. P. R.*, 1888-9, 5:269-270; Minot: *North American Review*, 1895, 160:222 ff.; Lehmann: Auberglaube und Zauberei, 2te Aufg., Stutt., 1908, pp. 458-65; and Donkin: *Nineteenth Century Magazine*, 1882, 12:132.

[2] Preyer: Die Erklärung des Gedankenlesens. Leipzig, 1886, pp. 49 ff.

[3] *Spectator*, January 30, 1869, pp. 136-7; Twain: "Mental telegraphy," *Harpers Monthly*, 1891, 84:100; Crookes: Part of the Presidential Address delivered to the British Association at Bristol, Sept., 1898. *Proceedings S. P. R.*, 1898, 14:3–4.

[4] *Vide*, Pfungst: Das Pferd des Herrn von Osten. Leipzig, 1907; or the English translation under the title of Clever Hans, which bears a preface by an American and an introduction by a German, both psychologists of the first rank. New York: Holt, 1911.

markable feats of mind-reading performed by little Beulah Miller of Rhode Island, Professor Münsterberg, who conducted a number of experiments with her, says, "I think everything can be explained through her subconscious noticing of unintended signs. But the signs which she receives are not noticed by her consciously. She is not aware of them; they go to her brain or to her subconscious mind and work from there on her conscious mind. . . . [Her] successes turn into complete failures as soon as neither the mother nor the sister is present in the room . . . The good results stop entirely when Beulah is blind-folded." [5]

If this is a legitimate explanation the psychological laboratory might be expected to provide methodical proof of the influence of "subliminal impression." Some of the professional literature, indeed, does supply support to this principle, in statements from psychologists of high rank, in reports of investigations of which the support is a by-product, and in a few reports of investigations in which the problem was directly attacked.

The first good account of this hypothesis with some notice of its older exponents and its warrant in fact was given by Sir William Hamilton. [6]

Lipps speaks of unconscious psychical stimuli.[7]

Carpenter says, "There seems no inherent improbability in the supposition that the power of intuitively interpreting the indications involuntarily furnished by expression of the countenance, gesture, manner, etc., so as to divine what is passing in the mind of another person, may be greatly intensified" [8] in certain mental conditions or in certain individuals; and he has given wide currency to Laycock's concept of "Unconscious Cerebration." [9]

James R. Angell [10] points out that in perception we are always aware of the "fringe" or background of consciousness, of sense activities other than those we speak of as being perceived, and that this "fringe" constitutes a consciousness of particular things present to the sense.

Külpe [11] shows that for the adult consciousness a content may be

[5] Münsterberg: Psychology and Social Sanity. New York: Doubleday, 1914, pp. 162-4.

[6] Hamilton: Lectures on Metaphysics. Boston, 1859, pp. 241 ff.

[7] Lipps: Grundtatsachen des Seelenlebens. Bonn, 1883, S. 125.

[8] Carpenter: Mesmerism, Spiritualism, &c., 1895, pp. 54–55.

[9] Carpenter: *Quarterly Review,* 1871, 131:316 ff.; also Principles of Mental Physiology. New York, 1886, ch. XIII.

[10] Angell: Psychology, New York, 1909, p. 151.

[11] Külpe: Outlines of Psychology. London, 1901, p. 291.

analyzed into "unconscious," yet psychic, components, and that after analysis the parts can be recognized by introspection. This "unconsciousness" is a characteristic of attention, not of "fusion" as is the case in hearing a "clang" of musical sounds.

Jastrow and Nuttall [12] performed experiments to determine the existence of a magnetic sense, and found in their first series of 800 experiments that the faintly audible molecular crepitation and click caused by magnetising and demagnetising the magnet had been unconsciously used as a basis for forming the judgment by the reagents, whose Right cases indicated an influence beyond chance.

Stout [13] grants a mental process taking place below the threshold of consciousness and says, "There is no reason why the slight sensory indications operative in so-called thought-transference should be exclusively muscular or auditory. And it is quite probable psychologically that sensory indications may operate without being discerned by the person whom they influence."

Peirce and Jastrow [14] found in experiments on sensible discrimination that subliminal differences in brightness, although unrecognized, were clearly effective in determining the judgments.

Donaldson,[15] after quoting the immediately preceding research, says, "Differences too small to be discriminated may still influence our reactions, and it is thus seen that among effective stimuli there must also be included those which we do not recognize."

Eckener [16] showed that when a minimal sound becomes subjectively inaudible, the reagent is still in many cases able to tell when the stimulus is interrupted, and observed that a stimulus can still affect consciousness although quite unperceived.

Stratton [17] points out that since the weights of 100 and 102 grams are absolutely indistinguishable, when hefted, and the weights of 102

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[12] Jastrow and Nuttall: *Proc. Am. S. P. R.*, Series 1, 1:116-126.

Concerning this series of experiments, Tuckett (Evidence for the Supernatural, p. 505) says: "One could not have a better example both of the importance of subconscious impressions received through the senses, and of the difficulty of excluding fallacies in telepathic experiments."

[13] Stout: *Hibbert Journal*, 2:47, 62.

[14] Peirce and Jastrow: On Small Differences of Sensation. *Memoirs Nat'l Acad. Sci.*, Washington, 1884, 3:73–83.

[15] Donaldson: The Growth of the Brain. London, 1895, p. 292.

[16] Eckener: Ueber die Schwankungen in der Auffasung minimaler Sinnesreize. *Philos. Stud.*, 1892, 8:364–5.

[17] Stratton: Experimental Psychology and its Bearing upon Culture. New York, 1903, pp. 84 ff.

and 104 grams are likewise indistinguishable, each pair of impressions feeling just alike, and since the weights of 100 and 104 grams are clearly distinguishable (the threshold of distinguishable difference being 2.5 grams for a weight of 100 grams) "the sensations arising from the weights of 100 and 102 grams are really different, although the difference is imperceptible."

Moore [18] in an interesting and important research had his reagents, in one series of experiments, observe series of five geometrical figures, presented simultaneously at the rate of one-half second (exposure one-fourth second) and after a varying number of repetitions, always less than sufficient for certainty, give judgment as to whether there was a common figure in the successive series. The reagents were often able to state with a feeling of certainty that there was a common figure but they had no idea as to its form. Sensations, indeed, had been received, competent to render correct judgment, but there was no recognition of the figure upon which the judgment, under conditions of satisfactory perception, would be based.

The writer [19] in tachistoscopic experiments which made it incumbent upon the reagent to record as many letters as possible from an exposure of a 12-letter-card for .085 seconds, found that letters not recognized as seen often insinuated themselves properly into the record. An incipient or a subliminal impression resulted in more or less vague imagery of the letter, and from this alone the record was tentatively made.

Bergson [20] reported that a hypnotized boy could read Arabic figures reflected in his eye when their total heights could not have been more than 1/250 of an inch.

Mrs. Verrall [21] performed a series of 400 experiments in card-guessing in which she drew the face of the card along the inner edge of the left thumb, and greatly exceeded the limits of chance in the number of Right cases both on the number of pips and on the whole card. Although during the first hundred experiments her judgment took conscious account of the feeling from the thumb, for the remainder she was

[18] Moore: The process of abstraction, *University of California Publications in Psychology*, 1910, 1:134–5.

[19] Coover: Formal Discipline from the Standpoint of Experimental Psychology, *The Psychological Monographs*, 1916, No. 87:86, 87, 91.

[20] Bergson: De la simulation inconsciente dans l'état d'hypnotisme. *Revue Philosophique*, 1886, 22:527.

[21] Verrall: Some experiments on the supernormal acquisition of knowledge. *Proceed. S. P. R.*, 1895, 11:175–185.

not conscious of the process and the cards regularly came to her mind as visual impressions. In another series of 280 guesses while looking at the back of the card her success was equally marked, although upon close examination no marks nor indications on the backs could be found; and since the success fell off in guessing with closed eyes, she suspected that the stamping of the pictures and pips on the cards may have produced indications on the backs, which were subliminally perceived.

Dr. Sauvaire [22] suggested to a hypnotized girl that there was a portrait on the back of a certain card (the King of Clubs) and that she would still see the portrait after she was awakened. This she did, after searching out the card from a shuffled pack, by means of some mark of identification not visible to other persons; and, since she repeated the feat of finding a card in another pack bearing the same portrait, which was also the King of Clubs, the mark of identification must have been a subliminal impression (tactual or visual) of the face of the card which at no time was turned toward her, or toward the light, Dr. Sauvaire suspecting that she saw through the card.

Perhaps reference should also be made to a few classical illustrations of the influence of subliminal or unnoticed impressions upon reproductive processes, as well as upon judgment.

Scripture [23] (1) showed pairs of words and Japanese signs, as Hana-AB, and Blume-AB. Then the words were shown alone and the reagent recorded any word that seemed associated with them. In this case Hana often called up Blume, and Blume Hana; *i. e.*, a word would come into mind through an unconscious and absent connection.

(2) He also showed to reagents pictures and small signs; the latter were not seen clearly since the picture was shown for but a brief interval. Then these signs were shown. Many images associated with them corresponded to the pictures upon which they were printed; *i. e.*, an unnoticed element of the situation, when focal in attention, revived the situation.

Jerusalem [24] reports an interesting case of a "Feldmarshalllieutenant" who while at work with his maps at a table had a sudden vision of a girl leading an old man; details of vision clear, whole scene persistent. The garb suggested the Orient, and then he remembered that he had

[22] Sauvaire: Observations, d'hyperesthésie des sens dans l'état hypnotique. *Revue Philosophique,* Paris, 1887, 23: 333-5.

[23] Scripture: Ueber den associativen Verlauf der Vorstellungen. *Phil. Stud.,* 1892, 7: 50–146.

[24] Jerusalem: Ein Beispiel von Association unbewusste Mittelglieder, *Philosophische Studien,* 1894, 10: 323–5.

seen them 30 years before; but he could not account for the vivid and persistent appearance of the scene until he discovered the scent of the *Pyrola uniflora* in the flower-glass, a flower he learned in the Orient at the time of the scene.

And Dr. Thomas,[25] in Ohrdruf, gives another example of the same power of unnoticed stimuli to awaken forgotten memories and call them out into the clear field of consciousness where they are regarded as intruders and trespassers. While a student in Berlin, in 1861, one day in late autumn on his way to college just over the Ebertsbrücke his train of deep thought was suddenly interrupted by vivid scenes in Hüttenthal where he used to live many years before. Having to hurry to get to college in time, he recovered his train of thought and dismissed the experience from his mind. The next morning at the same place the visions again intruded themselves, and so singular was the experience that he was impelled to stop and investigate the cause; after retracing his steps to the point by the bridge where the experience occurred, he discovered the odor of Bohemian coal, which had been commonly used in Hüttenthal, coming from a ship.

In the psychological laboratory a few investigations have been made to determine directly whether subliminal visual stimuli influence judgment. Dunlap[26] found some indication that imperceptible shadows exercised an influence upon the judgment of distance. The familiar Müller-Lyer illusion-figure was employed. In the preliminary series, the horizontal-line segments were drawn in black ink 1 mm. in width on a screen of white bristol-board; the oblique lines (arrow-head formed) were supplied in shadows cast by a variable light behind the illumined card. Shadows of a square and a circle were employed to determine the necessary depth of shadow; rear illumination was decreased until the reagents could not tell whether the figure up was a square or a circle, before the experiments began. There was evidence that the illusion was caused by the imperceptible shadows, although certain errors were known not to be excluded. In the main series an electric arc light was used, by the aid of a system of mirrors, for both general illumination and shadows; the latter being cast by fibers in a frame screened from the reagent, and accurately measured as to intensity by the use of an episkotister. Great care was taken by repeated testing to insure the

[25] Thomas: Ein weiteres Beispiel von Assoziation durch eine Geruchempfindung als unbewusstes Mittelglied. *Zeitschrift für Psychologie,* 1896, 12: 60-61.

[26] Dunlap: Effect of imperceptible shadows on the judgment of distance. *Psychological Review,* 1900, 7: 435.

imperceptibility of the shadows. The author concluded that his results, from four reagents, "strongly suggest that the imperceptible illusion-figure is active in producing psychical results, but for the sake of conclusiveness additional experiments should be carried out" under modified conditions, since the importance of the question requires "the maximum of careful investigation before we dare call it settled."

Titchener and Pyle [27] reported the results of a repetition and extension of Dunlap's work, likewise using the Müller-Lyer illusion-figure, and concluded "that these subliminal shadows, even raised almost to the limit of perceptibility, have no influence whatsoever upon the judgments of distance passed by five observers. . . . It follows from the whole investigation that if the subconscious is to be received into experimental psychology at all, it must find some other means of access than these imperceptible shadows."

Helen M. Mangro and Dr. Margaret F. Washburn [28] following the suggestion of both preceding investigations, conducted 1,370 experiments with the Müller-Lyer illusion-figure, substituting, however, faint penciled lines for the shadows. The figures were held "at such a distance from the observer that the pencil lines were just not visible." The tests were made on ten fairly practiced observers. "It seems improbable," they concluded, "that the lines . . . had any influence upon the judgments, except possibly in . . . two cases. Our results are thus in accord with those of Titchener and Pyle."

Perhaps the most suggestive experiments, however, from the psychical research point of view, were carried out by Sidis,[29] who took his departure from phenomena found in the dissociation of personality. "Now," he says, "if the hyperaesthetic, subwaking self and the waking self-consciousness, their inter-relations and their inter-communications, subsist also in normal life, as they most certainly do in the states of hypnosis, automatic-writing and crystal-gazing—if they subsist, I say, also in the life of every man, we ought to find it out by experiments. *We ought to find that sensory impressions that lie outside the range of sensibility of the waking self, but within the range of the sub-waking self, that such sensory impressions will still be transmitted to the primary*

[27] Titchener and Pyle: The effect of imperceptible shadows on the judgment of distance. *Am. Jr. of Psychology,* 1907, 18: 388.

[28] Mangro and Washburn: The effect of imperceptible lines on the judgment of distance. *Am. Jr. of Psychology,* 1908, 19: 242.

[29] Sidis: The Psychology of Suggestion. New York, 1898, ch. XVII, pp. 162 ff.

self.[30] The guesses of the subject must rise far above the dead level of chance. . . . And such is actually the case." (p. 165).

"The first set of experiments I made on myself. My right eye is amblyopic; it sees very imperfectly: for it, things are enshrouded in a mist. When the left eye is closed and a book is opened before me I am unable to tell letter from figure; I see only dots, rows of them, all indistinct, hazy, oscillating, appearing and disappearing from my field of vision. When a single letter or figure is presented to my right eye, I see only a black dot, as a kernel surrounded by a film of mist.

"I asked Mr. B. to make twenty-five slips and write down on each slip four characters—letters, figures, or both—in different combinations, but so that in all the twenty-five slips the number of letters should equal the number of figures. When a slip was presented to my right eye, the other being closed, I had to guess which of the characters was letter and which was figure. When the first series of twenty-five was ended the slips were shuffled, and a second series began. . . . I made two groups of experiments with two series in each group. Each series consisted of a hundred experiments, so that there were four hundred experiments in all." This class of experiments was called Class *A*.

RESULTS.

First Group	Experiments	General Character Guessed		Chance	Secondary Sight %
1st Series	100	68		50	18
2d Series	100	72		50	22
Second Group					
1st Series	100	70		50	20
2d Series	100	76		50	26
Totals	400	286	71.5%	50	21.5

"In the first series of the first group, out of one hundred characters sixty-eight were correctly guessed. Since there were only two guesses—letter or figure—50% must be subtracted, as so much might have been due to mere chance (we shall find, however, from our other experiments that the percentage subtracted is too high); 18% thus remains in favor of messages coming from the secondary self—in other words, 18% is left in favor of secondary sight.

"Out of four hundred experiments made, the general character was guessed two hundred and eighty-six times, which gives 71.5%; subtracting 50%, we have 21.5% in favor of secondary sight.

[30] The italics are inserted by the writer.

"Figures often speak more eloquently, more convincingly, than volumes. The results of the correct answers as to the general nature of the character due to secondary sight are far below the actual one, for in subtracting 50% we subtracted too much, as our experiments will show farther on; still they were so striking that I communicated them to Professor James and he was kind enough to encourage me in my work, and advised me to pursue the inquiry further in the same direction."

Then followed experiments somewhat modified:

In Class *B*, five letters (*A, B, E, N, T*) and five digits (*2, 4, 5, 7, 9*) were chosen, and each capital or digit was written on a separate card. Sidis was again reagent, looked with his amblyopic eye, and guessed each time one of those ten particular characters. "Now here each guess could be either general or both particular and general, or fail altogether. When . . . for instance, I took *5* for *7*, or *E* for *N*, I guessed rightly the general nature only of the character shown. When I gave the correct name I guessed, of course, both the particular and the general nature. When, however, I mistook a letter for a figure or a figure for a letter, I failed, and failed completely."

In Class *C*, the same ten characters were put down, one on each card, "in faint outlines," and each of eight subjects with normal vision was required to guess the particular letter on the card which was presented at such a distance "that the character was outside his range of vision; he saw nothing but a mere dot, blurred, and often disappearing altogether. The subject was told that there were ten cards in the pack, that the number of letter cards was equal to that of the figure cards, *but he was not told the particular names of the characters.* Each time a card was shown the subject had to give some particular name of character he took that dot to be. 'They are all alike, mere blurred dots,' complained the subjects. 'No matter,' I answered; 'just give any letter or figure that rises in your mind on seeing that dot.'"

In Class *D*, the letters (hereafter, *B, H, K, U, Z*) and digits were printed on the cards in heavy Gothic type about 6.3 mm. in height, the lines 1 mm. in thickness, and for the first time were well formed and uniform. These experiments were made with 20 quite different subjects who were told that there were five figure cards and five letter cards, but were not told the particular names of the characters. Two series with ten experiments each were given each subject separately. "The subject was placed at such a distance from the card that the character shown was far out of his range of vision [no distance is given]. He saw

nothing but a dim, blurred spot or dot." The subject was required to name some particular character which that particular dot shown might be. "It is nothing but mere guess," commented the subjects.

In Class *E* the subjects and procedure were the same; but the subjects were told what the particular characters were.

In the following table the results are brought together:

TABLE LVIII. SIDIS' RESULTS.

	No. of Guesses	Right Gen.	Right Par.	Per cent: Right Gen.	Right Par.	Chance Gen.	Chance Par.	Secondary Sight* Gen.	Secondary Sight* Par.
Class *B*	400	273	188	68.2	47	50	10	47.6	37.0
Class *C*	400	255	92	63.7	23	50	2.8	40.5	20.2
Class *D*	400	270	103	67.5	25.8	50	2.8	43.8	22.9
Class *E*	400	291	139	72.8	34.7	50	10	43.7	24.7

*These values under Secondary Sight need revision. On p. 372, in the Appendix, Sidis gives his explanation of the formula on the succeeding page which is used throughout; and it is evidently wrong. It follows, with application to the results in Class *B*:

y = the correct general guesses due to secondary sight
x = the correct particular guesses due to secondary sight
p = the correct general guesses due to chance
p_1 = the correct particular guesses due to chance

$(p + y) + (p_1 + x) = 68.2$ (The % Right as to general character in Class *B*) [1]
$(p_1 + x) = 47$ (The % Right as to particular character) [2]
$(p + y) = 21.2$ [3]
$p = y$, $\therefore\ y = 10.6$ [4]
$p_1 = 10$ [5]
$x = 37$ (Sec. Sight, Particular) [6]
$y + x = 47.6$ (Sec. Sight, General) [7]

"Now, in equation [3], p is 50 per cent, because each guess has only two alternatives, letter or figure [thus far all is well]; in other words, $p = y$ [here lies the fallacy!]; hence, $y = 10.6$." (p. 372).

By definition, p is the correct general guesses due to chance, expressed in per cent of the whole number of guesses; but if there are any correct guesses due to "secondary sight," this p must be calculated upon the remaining number of guesses; it is never 50%, and it has no constant relation to y. The probability of R guesses by chance must be discriminated from the per cent of all the guesses which are right by chance. Let us call it p_y (p sub y); since there are but two general classes of characters the probability of R guesses by chance is 50% ($p_y = 50\% = .5$); and since the only field available for R general guesses by chance is $[1 - (x + y)]$,

$$p = p_y [1 - (x + y)].$$

Another error in the formula is to be found in equation [1]; it should not

"As remarked above, the subjects often complained that they could not see anything at all; that even the black, blurred, dim spot often disappeared from their field of vision; that it was mere 'guessing'; that they might as well shut their eyes and guess. How surprised were they when, after the experiments were over, I showed them how many characters they guessed correctly in a general way, and how many times they gave the full name of the particular character shown!

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contain p_1, since if a guess of a particular character is right by chance, the character's general class was either correctly determined by "secondary sight," in which case it is included in y, or correctly guessed by chance, in which case it is included in p. All p_1, then, is included in $(p + y)$. Hence the first equation should be

$$p + x + y = \text{Right General Guesses.}$$

Then p_1, like p, must be discriminated from the probability of guessing by chance a particular character, since it represents the per cent of the whole number of guesses that have been right as to the particular character by chance. Let the probability be p_x; it, in this case of 10 alternatives, is equal to 10% or .1, or, since it bears a definite relation to p (every R general guess due to chance stands one chance in five of also being a R particular guess), 20% of p or $2p$, in case the general character has not been acquired by "secondary sight"; in case it has, there are five alternatives and $p_x = .2$, applying to y; consequently

$$p_1 = .2\ (p + y).$$

This revision of the formula, applied to the data of Class B, changes the results:

	$p + x + y = .682$	[1]
	$p_1 + x = .47$	[2]
Subtracting [2] from [1]	$p + y - p_1 = .212$	[3]
	$p_1 = .2\ (p + y)$	[4]
Substituting [4] in [3]	$.8(p + y) = .212$	[5]
	$p + y = .265$	[6]
Subtracting [6] from [1]	$x = .417 = 41.7\%$	[7]
	$p = .5[1 - (x + y)]$	[8]
	$= .5 - .2085 - .5y$	[9]
Substituting in [6]	$y - .5y = .265 - (.5 - .2085) = -.0265$	[10]
	$y = -.053 = -5.3\%$	[11]

We find in favor of "secondary sight" for the particular character, 41.7% of the whole number of guesses; but the negative quantity of y, apparently a chance deviation, seems competent to show that, in this series at least, there was no "secondary sight" for the general character alone, in operation.

In appraising the results of these experiments, therefore, the values deduced from the formula must not be taken at face value, owing to errors in the formula. y was not 10.6%.

"Now these experiments tend to prove the presence within us of a secondary sub-waking self that perceives things which the primary waking self is unable to get at. The experiments indicate the interrelation of the two selves. They show that messages are sent up by the secondary to the primary self.

"Furthermore, the results seem to show that, in case the particular message fails, some *abstract general account* of it still reaches the upper consciousness. An inhibited particular idea still reaches the primary self as an abstract idea. *An abstract general idea in the consciousness of the waking self has a particular idea as its basis in the sub-waking self.*" (p. 171).

"While the above-mentioned experiments on secondary sight were under way another set of experiments was carried out by me the purpose of which was to tap directly the suggestibility of the secondary self, and to find out the influence the subconscious has on the primary consciousness.

"The mechanism of the experiments was as follows: On slips of paper I made a series of complicated drawings. Each slip had a different pattern. The subject had to look at the pattern of the drawing for ten seconds, and then the slip was withdrawn and he had to reproduce the drawing from memory—a task extremely difficult. It took him about fifteen seconds and more before he could make anything bearing the slightest resemblance to the drawing shown.

"When he finished the drawing an elongated cardboard with eight digits pasted in a row was shown to him and the subject had to choose whichever digit he pleased. Now, on the margin of each slip was written a digit contained in the number of digits on the cardboard from which the subject had to choose. The subject, not having the slightest suspicion of the real purpose of the experiments, being *perfectly sure* that the whole matter was concerning imitation of the drawings and being assured by me that the choosing of the digits on the cardboard was nothing but a device to *'break up the attention'* in passing from one drawing to another, and being besides intensely absorbed in the contemplation and reproduction of the drawing, which was extremely complicated—the subject, I say, wholly disregarded the figure on the margin—he did not even notice it. I so fully succeeded in allaying all suspicions and distracting the attention of the subjects that when Professor James interrogated one of them, an intelligent man, he was amazed at the latter's complete ignorance as to what was actually going on."

". . . The upper primary self, being completely absorbed with the drawing, did not notice the figure, or, if it did, it soon learned to disregard it, because he thought it insignificant, and because it would only distract his attention. But although the figure was not noticed and fully disregarded (a fact I was careful to find out from the subjects in an indirect way), it still impressed the sense organ, reached the secondary self, which took it as a suggestion, sending it up as a message to the primary self or personality and influencing the latter's choice. This choice suggestion is strikingly analogous to post-hypnotic suggestion. . . .

"Before giving the results let me say a few words as to the classification of the experiments. When I started my first experiments of this kind a suspicion crept into my mind that it might be fully possible that in case a suggestion given did not succeed it might still succeed *partially* as mediate suggestion, by arousing some association which will be obeyed. For instance, in giving *6* as a suggestion, *6* itself might not be chosen, but some number that succeeds or precedes it, such as *5* or *7*, or possibly a numeral next to the suggested one in place, say *1* or *2*, for I arranged my figures on the cardboard in such a way as to break up the natural succession of the digits. I was therefore careful to make two separate classes for these two kinds of association suggestions—namely, *suggestion by locality* and *suggestion by numbers*, which we may term locality and number suggestions. The results of my experiments showed me the mediate suggestion was here of but little importance.

"I made 1000 experiments and operated with 20 subjects, of which 16 were fresh ones, not having taken part in any of my other experiments.

"The figures on the cardboard were arranged thus: *2 6 4 7 1 5 3 8*.

RESULTS.

Out of the 1000 experiments the number which had been on the drawing was guessed 394 times. Since the chance of guessing right is 12.5% or .125, x (the influence of causes besides chance) $= .394 - [.125(1 - x)] = 30.7\%$:

$$\begin{aligned} x &= .394 - [.125(1 - x)] \\ &= .269 + .125x \\ x - .125x &= .269 \\ .875x &= .269 \\ x &= .307 = 30.7\% \end{aligned}$$

(Sidis' formula, p. 377, makes $x = 32.1$).

"How shall we explain the fact that in our experiments the percentage of correct guesses is far above the one due to chance alone? Two

theories are on the field to account for this fact: one is the well-known unconscious cerebration, and the other is my own point of view, or what I may call the psycho-physiological theory.

"On the theory of unconscious cerebration, each figure shown outside the range of vision made an impression on the retina. This impression was transmitted to the sensorium, to the central ganglia of the brain, the occipital lobes, exciting there physiological processes that are not strong enough to rise above the threshold of consciousness. In short, each figure stimulated the peripheral sense organ, giving rise to a central but unconscious physiological process. Now, according to the theory of unconscious cerebration, it was this unconscious physiological process that helped the subject to form correct guesses.

"The psycho-physiological theory, while agreeing with the theory of unconscious cerebration as to the physiological account, makes a step further. Each figure certainly made an impression on the peripheral sense organ and induced central physiological processes, but these processes had their psychical accompaniments. Far from being mere mechanical, unconscious work, these physiological processes were accompanied by consciousness; only this consciousness was present not to the upper, but to the lower subconscious self. . . . In short, the percentage of correct guesses in our experiments can not be accounted for on the theory of unconscious cerebration; there must therefore have been *conscious perception.*

"Furthermore, to have a correct general idea of a scarcely perceptible dot as being letter or figure, there must evidently be some perception of the particular traits of the dot; there must be a subconscious perception of the particular letter or figure." (pp. 177-8).

I have quoted Sidis liberally for the reason that his experiments are so highly suggestive of ways in which judgment might be influenced, and that they offer a means of studying psychical phenomena with psychological profit. Professor James wrote an introduction to the book, in which he says: "By other [these] ingenious experiments Dr. Sidis tries to show that the 'subliminal' or 'ultra-marginal' portion of the mind may in normal persons distinguish objects which the attentive senses find it impossible to name. These latter experiments are incomplete, but they open the way to a highly important psychological investigation." (p. vi).

If we treat the data in Classes *B, C, D,* and *E,* with revised formulæ we find values which we may compare with the probable per cent (p),

and the limit of chance deviation (L).[31] In the following table, x/L and y/L indicate how many times the "secondary sight" value (x or y) contains the Limit of Chance Deviation, and are therefore measures of the reliability of the x and y values; x is the influence beyond chance causing R guesses of the particular character; y is the influence beyond chance causing R guesses with respect to the general character of the symbols:

TABLE LIX.

Sidis' Values Revised.

	Particular Right.				General Right.			
	x	p	L	x/L	y	p	L	y/L
Class B	41.7	10	6.4	6.5	−5.3	50	10.6	0.0
Class C	20.3	2.8	3.5	5.7	7.1	50	10.6	0.7
Class D	23.3	2.8	3.5	6.7	11.7	50	10.6	1.1
Class E	25.2	10	6.4	3.9	20.2	50	10.6	1.9

Concerning the influence working for Right guesses with respect to the general character of the symbols, the table indicates that in Classes B and C it is negligible if not absent, and in Classes D and E it is small, but, perhaps, unquestionable. Since it is the y values upon which the hypothesis of an elaboration or classification, below the threshold of conscious activity, of particulars received as subliminal impressions, depends, it would be interesting to know whether the digits and capitals differed in any noticeable respect in the font of Gothic type from which they were printed; the values of y become distinguishable from chance variation only in the series in which these printed letters were used (D and E). Should the digits be narrower or shorter or lighter than the capitals,[32] for instance, the classification would not need to be assumed to depend upon a subliminal perception of the particular characters and consequently would not necessarily involve a more subtle process than the suggestion effected by the subliminal impression of the symbols which leads to Right Guesses of the particular characters. This matter of subliminal elaboration needs further careful investigation.

[31] $L = 3\sqrt{\frac{2pq}{n}}$.

[32] We reproduce here the same letters and digits as were used, in a similar font of Gothic type. The digits are shorter, narrower, and lighter in the line than the capitals:

B H K U Z 2 4 5 7 9

Fig. 1. Characters used by Sidis.

The influence working for Right Guesses of the particular symbols, on the other hand, is definite and unquestionable. But, as Dunlap suggested, the influence of subliminal impressions upon the judgment is an hypothesis of so great importance, for psychology in general, and for psychical research in particular, that we must be very careful in verifying the fact. Many subtle errors will inevitably be encountered in so difficult a research, and safety necessarily lies in caution. The precise degree of invisibility, since there are no real thresholds outside of conventional concepts, needs to be determined, for a visual stimulus, and the fluctuation of acuity and the effect of repeated trials upon accommodation must be reckoned with in establishing and maintaining conditions of subliminal impression. Introspective check would be of the highest value, if the reagents are capable of accurate introspection; and the integrity and the scientific attitude of the reagent must be insured. Sufficiently full and accurate descriptions of the conditions of experimentation should be published in the report of the work to enable others to pass critical judgment upon the results or to repeat faithfully the investigation.

These suggestions for caution are not intended as a criticism upon Dr. Sidis' interesting and original experiments in the Harvard laboratory, for his work was that of the pioneer and altogether admirable, but as an intimation of the relation our own investigation bears to his, since we sought a more definite control of experimental conditions. Our estimate of the importance of his work is indicated by the fact that our investigation was patterned after his.

Before turning to our own work, however, we should notice the report of another research which followed Dr. Sidis.

Stroh, Shaw, and Washburn [33] performed three series of experiments, in the psychological laboratory at Vassar College, which they describe as follows: "In the first of these, a procedure like that of Sidis was followed. The cards used bore each of them one of the first ten letters of the alphabet, and they were held at such a distance that the observers could barely detect the letter as a faint spot on the card. The observer was told that the letter on the card was one of the letters from *A* to *J*. In a large number of experiments, then, the probability would be that one-tenth of the guesses at the letters would be correct, if the guessing was not subject to any influence.

[33] Stroh, Shaw, and Washburn: A study in guessing. *Am. Jr. of Psychology*, 1908, 19:243–245.

"In the second series, the conditions were rendered more difficult by enclosing the letters in rectangles. It was thus made almost impossible to be guided in guessing by the general bulk of a letter,—as, for instance, *B* might in the first series be distinguished from *I*.

"In the third series, the letters were whispered instead of being shown on cards. It was found necessary in this series to rule out every experiment where the observer heard the slightest sound from the whispering. If anything at all were heard it often caused the letter to be recognized, especially such letters as *C, G. H,* and *J*. The experimenter would therefore give the observer a 'Ready' signal, and then whisper the letter so softly that no sound whatever could be heard at the distance at which the observer sat."

RESULTS.

The probability for Right guesses by chance in each of the three series is 10%. In Series I, individual reagents made from 50 to 330 guesses each, and their per cents of R cases were: 16, 16, 22, 24, 32, 33, 38, 38, 46, 48, 51, 55, 74. Some of the observers "obtained so high a percentage of R guesses as to suggest that they must have been almost able to read the letters, although they declared in good faith that they could not."

In Series II, the reagents made from 50 to 240 guesses each, and their per cents of R cases were: 8, 18, 21, 24, 34, 37, 63, 67. In this series "The possibility of reading in the ordinary sense was much less, . . . yet two of the observers guessed right in more than half of the cases."

In Series III, the reagents made from 100 to 600 guesses each, and their per cents of R cases were: 10, 12, 13, 16, 16, 19, 23, 24, 31, 39. In this series, "where the letters were whispered, since every case in which the observer heard the slightest sound of the whisper was ruled out, the conditions should have made ordinary perception impossible. It is noteworthy that although no observer fell below 10% of R guesses three reagents made little above that amount. Yet two reagents . . . show that their guessing must have been somehow influenced quite decidedly in the right direction, and the others also give evidence of such influence, though in a less marked degree."

Disregarding individual differences, and aggregating the guesses and the R cases for the three respective series, we get results shown in the following table; x represents the influence beyond chance;[34] p, the

[34] $x = \%R - [p(1 - x)]$.

probability for R cases by chance; L, the limit of chance deviation;[35] and x/L shows the reliability or value of the x magnitudes (1 being zero value):

TABLE LX.

Results of Stroh, Shaw, and Washburn.

Series	Number of Reagents	Number of Experiments	%R	x	p	L	x/L
I.	13	1816	39.6	32.9	10	2.9	11.3
II.	8	890	44.6	38.4	10	4.3	8.9
III.	10	2556	26.0	17.8	10	2.5	7.1

The last column of the table shows that the x values are many times the range of chance deviation, and that they decrease down the series; considering the amount of these values, and the fact that of the 31 individual sets but one falls below the probable chance value (10%) in R cases, we must recognize a strong and fairly general influence beyond chance in the guessing, which, presumably, is the effect of subliminal impression. The investigators concluded: "Our results, then confirm, on the whole, those of Sidis and show that with certain observers at least judgments may be influenced in the direction of correctness when the observer is unconscious that any such influence is present. Whether this effect is due to a secondary self with superior senses, as Sidis believes, or to a physiological result of the stimulus, too slight to affect consciousness on its own account, as it were, is a question to which our experiments can furnish no answer."

But perhaps we are too liberal in our estimates, toward the hypothesis of subliminal influence; chance may not be limited to the theoretical 10% in the Series I and II, since the reagent might, voluntarily or involuntarily yet consciously, make use of some general aspects of the indefinite stimulus, and thus raise his chances for R cases. For example, suppose the letters were traced, as were those of Sidis, in Class *C*, in the form of capitals, then we might have the following classification:

B E H, full, heavy appearance of the blurred stimulus, Chance..... .33
C G D, rounded, open, appearance of the blurred stimulus, Chance..... .33
I J, long, narrow, appearance of the blurred stimulus, Chance..... .50

If we assume that the respective 10 letters were exhibited equally often, and the letters *A* and *F* remain wholly indefinite as stimuli (Chance .10),

[35] As before, (pp. 85, 185), $L = 3\sqrt{\frac{2pq}{n}}$.

then, for a long series of experiments the chance of R cases would be raised to 32%.[36] Of course, this method of guessing may not have been generally used. The question is, was it used at all? If so, then our x values for Series I and II are too large. That this cause has been operative seems to be indicated by the fall of the x/L value in Series II below that in Series I, and it is doubtful whether the square inscribing the letter, in Series II, would necessarily shield the stimuli altogether from similar classification, although its influence would obviously be in this direction. However, since all conscious impression was ruled out in Series III, which shows an undoubted influence beyond chance, the decrease in the x/L value in Series II might have been caused by a lowering of the stimulus proper further below the threshold of conscious perception, thus decreasing somewhat its effectiveness.

Careful introspections and modification of stimuli may, in future researches, definitely settle the questions raised by these two investigations which attacked directly the problem of the influence of subliminal impression upon judgment. In the face of the results the presumption lies much in its favor; but its status will be better assured when conditions, amounts, and interpretations, perhaps, can be confidently stated.

[36] For A and F, $.10 \times 2 = .20$
B D H, $.33 \times 3 = .999$
C G D, $.33 \times 3 = .999$
I J, $.50 \times 2 = 1.000$
Total.... 3.199 Average, .3199 or 31.99%.

EXPERIMENTS.

In recognition of the importance, for Psychical Research, of the hypothesis of the influence of subliminal impression upon judgment, several series of experiments were devised and carried out, under conditions of experimentation suggested, directly or indirectly, by the investigations reviewed above. The general principle held in mind, however, was a more definite control of the conditions of experiment: (1) As to objective conditions, presentation of stimuli was controlled by the use of apparatus permitting the measurement of the time of exposure; the stimuli were uniform, being prepared by stamping or printing devices; and the distance of the reagent from the stimuli was made definite. (2) As to subjective conditions, the instructions made provision for an intermediate grade of judgment based upon partial perception; and introspections were required of some of the reagents.

The research was carried on during two school-years, 1914-15 and 1915-16, with the assistance of students selected from the large classes in General Psychology. There were 118 reagents; their vision safely, for our purpose, assumed to be normal. And 15,441 experiments, distributed according to experimental conditions in the following table, were performed:

TABLE LXI.

Experiments on Subliminal Impression.

	1914-15 Number of		1915-16 Number of		Total	
	Re-agents	Experiments	Re-agents	Experiments	Re-agents	Experiments
Div. I. Wirth-Tachistoscope	12	1120	26	3027	38	4147
Div. II. Wundt-Tachistoscope " (Foveal Vision)	15	1870	6	1350	21	3220
Div. III. " (Peripheral ")	12	954	28	4300	40	5254
Div. IV. (Miscellaneous).....					19	2820
Total	39	3944	60	8677	118	15,441

In Division I, the Wirthian tachistoscope was used and the time of exposure was cut down below the limen for perception: in Division II, the

Wundtian type of tachistoscope was used and with an exposure less than the reaction-time of the eye the distance was increased to minimal perception; in Division III, the stimulus exhibited in a Wundtian tachistoscope was presented to peripheral vision only; in the first two divisions it was presented, as in preceding investigations, to foveal vision.

Division I. With the Wirthian Tachistoscope.

In the first division of experiments, we used a Wirthian tachistoscope with a revolving disc 97.5 cm. in diameter. Adjustable shutters over a window on the edge of the disc permitted the slit through which the stimulus appeared to be varied in width for the purpose of varying the time of exposure, without changing the speed of rotation.[37] The letters *B H K U Z* and digits 2 4 5 7 9 were printed in black from rubber Roman type 21 mm. high[38] upon cards which slipped into a holder behind the disc, and by means of electrically operated traps dropped into position for exposure during the revolution of the disc. This position lay behind a metal screen, painted gray, uniform with the disc, which prevented distraction that might be caused by the visible movement of the shutters through the point of fixation.

Procedure.

The experimenter manipulated the stimulus cards behind the apparatus, rotated the disc by hand at the rate of once a second, gave a pre-signal about 15 seconds and a signal about 2 seconds before each stimulus. The reagents sat 4 meters from the disc, from which position the visual angle of the letters was 14.36′. At the beginning of the 1914-15 series, for reagents 1-12, the time of exposure was set at 9.5 sigma, but as practice effect increased the number of twice-underscored records it was decreased to 7.7σ, 5.7σ and 4.9σ, the limen for 50% twice-underscored records being about 7.7σ; the room was slightly darkened by the drawing of blinds, necessary to exclude shadows cast on the cards by the screen from cross-light, but the cards were well illuminated. In 1915-16, when the apparatus was stationed in another room which eliminated cross-light shadows and permitted the light from the windows

[37] From the center of the revolving disc to the calibration on the shutters over the center of the letter, the distance is 39.9 cm., making the circumference 250 cm. ($2\pi \times 39.9 = 250$). With rotation once a second, then, 250 cm. = 1 second; 2.5 cm. = .01 second = 10σ; 2 cm. = 8σ; 5 mm. = 2σ; etc.

[38] This reduced facsimile of the characters used as stimuli shows their form:

B H K U Z 2 4 5 7 9

Fig. 2. Characters used in the Wirthian Tachistoscope.

back of the reagents to fall directly upon the stimulus, the experiments began, for reagents 13-38, with an exposure of 10σ and decreased rapidly through 8σ, 6σ, 4.8σ, 4σ, 3.2σ, to 2.8σ, 2.6σ, and 2σ, with the limen of 50% twice-underscored records at about 4.6σ; 71.1% of all records from these reagents were made on exposures less than 3σ.

The reagents were instructed to look closely at the stimulus, immediately after exposure to record their perception of the letter or digit, or, in case none was perceived, to record a guess of a letter or digit. The guess was not to be formed by any scheme or rule but was to be purely spontaneous. In case part of the stimulus was sufficiently seen to influence the guess, as of a curved or a straight-line letter, the record was to be underscored once, since it is a partial perception or an inference with the range of error in guessing restricted; if the stimulus was perceived, the record was to be underscored twice; thus, the records of the spontaneous guesses alone remained without underscores. In Series I, the reagent understood that any letter of the alphabet or any of the ten digits might be exposed; in Series II, he was given a list of the 10 symbols to which he limited his guesses. The order of exposed letters and digits was worked out by chance before the experiment began, and modified in such a way that each letter and each digit appeared five times in each half of a set of 100 experiments.

The work was given in five sections, consisting of 7, 5, 9, 10, and 7, reagents respectively. Provision for group experiments was made for the purpose of determining individual variation in perception. The reagents worked independently, however, and in silence. Experimental hours for the reagent came one week apart, at the same hour of day, in the same room, and three or four sittings were required to finish the work. Reagents, with a few exceptions, took 100 or 150 experiments each. The time between experiments varied from one to two minutes; practiced procedure, as planned, required one minute. It is thought that fatigue was not induced.

Results.

Series I. In this series, the reagents did not know the particular symbols shown and consequently did not confine their guesses to those symbols. The probability for R cases by chance is 2.8%. The following table gives the whole number and the number Right for (*a*) perceptions (twice-underscored records), (*b*) inferences (once-underscored records), and (*c*) guesses; also the per cent R for the guesses; and the whole number of experiments; for the respective reagents:

TABLE LXII.

R Cases. Wirthian Tachistoscope. $p = 2.8\%$

	Reagent	Perceptions		Inferences		Guesses			Experiments
		No.	R	No.	R	No.	R	% R	
1914-15	*1*	29	27	49	26	22	2	9.1	100
	2	25	23	29	19	46	4	8.7	100
	3	..	..	..	..	30	4	13.3	30
	4	8	1	7	0	65	2	3.1	80
	5	7	5	8	4	45	2	4.4	60
	6	..	..	..	..	50	0	0	50
	8	3	2	12	7	15	4	27.0	30
	9	11	10	10	6	9	2	22.0	30
Totals	.....	83	68	115	62	282	20		480
%	.....		82		54		7.1		
1915-16	*13*	7	7	4	3	139	4	2.9	150
	14	12	12	10	3	128	9	7.0	150
	15	70	52	27	6	53	6	11.3	150
	16	20	17	46	8	84	2	2.4	150
	17	41	39	41	21	68	10	14.7	150
	18	40	31	20	4	60	7	11.7	120
	19	35	33	19	7	66	7	10.6	120
	20	2	1	6	2	66	3	4.5	74
	21	13	4	39	2	18	2	11.1	70
	22	18	17	11	9	10	3	30.0	39
	23	14	14	6	4	46	3	6.5	66
	24	33	32	7	3	60	5	8.3	100
	25	38	38	32	31	30	17	56.7	100
	26	19	19	28	22	53	7	13.2	100
	27	43	41	27	19	30	10	33.3	100
	28	15	14	2	1	83	14	16.9	100
	29	9	7	21	12	70	2	2.9	100
	30	41	36	28	8	31	2	6.4	100
	31	26	24	20	7	54	11	20.4	100
	32	68	67	24	16	8	3	37.5	100
	33	47	39	38	10	15	1	6.7	100
	34	4	4	40	4	56	1	1.8	100
	35	4	3	18	9	78	5	6.4	100
	36	13	11	8	3	74	9	12.2	95
	37	10	10	10	3	73	1	1.4	93
	38	8	7	18	5	74	6	8.1	100
Totals	.....	650	579	550	222	1527	150		2727
%	.....		89.0		40.4		9.8		
Grand Totals	.....	733	647	665	284	1809	170		3207
%	.....		88.3		42.7		9.4*		

* This value is not an average of the per cents listed in the %R column, but

The footings of the table show that of the 3207 experiments in this series, 733 yielded perceptions, and 665 partial perceptions or inferences, leaving 1809 guesses, of which 170 or 9.4% are Right as to the particular symbol. From other tabulations we learn that the guesses Right as to general character (letter for letter, and digit for digit) equal 53.8% as against 50%, the probable per cent by chance.

Leaving the central values and their significance for later discussion, we may note from the table that of the 34 reagents who contributed guesses, only 4 fall below the probable chance value in the per cent of R cases, that 7 are over 20%, and that 25 are over the largest chance value as calculated from the aggregate results ($p + 3\sqrt{\frac{2pq}{n}} = 2.8 + 1.65 = 4.45\%$). The distribution curve indicates that for over a fourth of the reagents, the results are probably chance values, as may be seen from the following distribution of the per cents of R cases:

0	6.4	10.6	20.4
1.4	6.4	11.1	22.0
1.8	6.5	11.3	27.0
2.4	6.7	11.7	30.0
2.9	7.0	12.2	33.3
2.9	8.1	13.2	37.5
3.1	8.3	13.3	56.7
4.4	8.7	14.7	
4.5	9.1	16.9	

Series II. In this series the reagents knew the particular symbols to be presented and limited their guesses to them. Probability of R cases by chance is 10%. The results tabulated as before are shown in Table LXIII.

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is calculated on the base of the total number of Guesses (1809), being the per cent of the aggregate of R guesses. In succeeding tables the corresponding value is similarly derived.

TABLE LXIII.

R Cases. Wirthian Tachistoscope. $p = 10\%$

	Reagent	Perceptions		Inferences		Guesses			Experiments
		No.	R	No.	R	No.	R	% R	
1914-15	*4*	6	3	2	0	12	1	8.3	20
	5	31	31	5	4	4	1	25.0	40
	6	28	28	7	6	15	3	20.0	50
	7	47	47	29	28	24	13	54.0	100
	8	12	9	39	7	19	2	10.5	70
	9	42	38	15	8	13	1	7.7	70
	10	57	56	25	15	18	2	11.1	100
	11	23	17	31	12	46	4	8.7	100
	12	33	28	28	14	29	7	24.2	90
	Totals	279	255	181	94	180	34		640
	%		91		52		18.9		
1915-16	*32*	0	0	0	0	50	5	10.0	50
	33	1	1	1	0	48	3	6.2	50
	34	0	0	1	0	49	4	8.2	50
	35	0	0	12	3	38	6	15.8	50
	37	0	0	1	0	49	4	8.2	50
	38	0	0	5	0	45	4	8.9	50
	Totals	1	1	20	3	279	26		300
	%				15.0		9.3		
	Grand Totals	280	256	201	97	459	60		940
	%		91.5		48.3		13.1		

We find that out of a total of 940 experiments, 280 were perceptions (twice-underscored records), and 201 were partial perceptions or inferences (once-underscored), which leaves 459 as guesses, of which 13.1% were Right in particular and, from other tabulation we learn, 57% were Right in general (letter for letter, and digit for digit) as against the probable per cents of R cases by chance of 10% and 50% respectively. The following is the distribution of the individual per cents of the Right guesses on the particular character:

6.2	8.7	15.8
7.7	8.9	20.0
8.2	10.0	24.2
8.2	10.5	25.0
8.3	11.0	54.0

Only three, or a fifth, of the fifteen values are definitely above the limit of chance for the aggregate, $(p + 3\sqrt{\frac{2pq}{n}} = 10 + 9.5 = 19.5\%)$,

and these fall in the 1914-15 part of the table. Records show that in the 1914-15 experiments, 79% of the guesses were made on exposures of 6σ, and 20% on exposures of 8σ, while the limen of 50% twice-underscored records was 7.7σ; and that in the 1915-16 experiments all the guesses were made on a 4σ exposure during a dark, murky day, the limen under ordinary light being 4.6σ; also that the former conditions of perception, which yielded but 31.1% of guesses, were much better than the latter conditions of perception, which yielded 72.6% of guesses. Consequently, the stimuli of 1915-16 were much below the threshold of perception, and we should, perhaps, consider the results of the 1914-15 experiments separately, although their paucity in numbers (180 guesses) will greatly decrease the reliability of the central measures: R cases on the particular character are 18.9%, and on the general character 65%, as against the probable chance values of 10% and 50%, respectively, while the respective chance-limits are 19.5% and 65.8%.

In the appraisal of the results of both Series I and Series II we should, perhaps, refine slightly our method. As was pointed out above, by Sidis, in our exposition of his results, to reckon the chance R cases upon the basis of all the cases (including those R cases due to other causes) gives us a value too large for the deduction from the total R cases to find the number of R cases due to extra-chance causes. If we apply the formula we derived for the purpose of testing Sidis' results,[39] we shall get truer values:

TABLE LXIV.

Summary of Wirthian Tachistoscope Results.

x = the % of R cases on the particular character due to extra-chance causes,
y = the % of R cases on the general character due to extra-chance causes,
p = the % of R cases to be expected by chance,
L = the % of R cases beyond which chance is not expected to deviate from p.

		Particular Right				General Right			
	Guesses	x	p	L	x/L	y	p	L	y/L
Series I ...	1809	6.8	2.8	1.65	4.1	1.0	50	4.98	0.20
Series II:									
Total	459	2.15	10	5.94	0.36	11.85	50	9.9	1.2
1914-15	180	7.4	10	9.48	0.78	22.60	50	15.8	1.4

[39] *Vide*, pp. 180 f. The general form of the formula is:

$$x + y + p = \text{R General}$$

$$x + \frac{p_x}{p_y}(y + p) = \text{R Particular}$$

in which R General and R Particular are the per cents of the Right guesses, and p_x and p_y express the probability of chance for R Particular and R General guesses respectively.

Concerning Series I it is definitely certain that there was some cause beyond chance working for R cases on the particular character, the x value being 4.1 times the chance-limit; and that there was no subliminal classification tending toward R cases on the general character of the stimulus, the y value being only .2, or 20%, of the chance-limit. Discussion on the possible nature of the extra-chance causes will be taken up after the quantitative results have all been inspected.

In Series II, we find, if we consider the total series, that the x value is only 36% of the chance-limit, if the 1914-15 part of the series, 78%, which is no certain indication that x is different from chance. But the y values are 1.2 and 1.4 times the chance-limit, respectively, indicating the presence of an extra-chance classifying process, though in no high degree.

Should we surmise that the approach of the x values toward their limits indicates the probability of the presence of extra-chance causes, and appeal to the individual cases showing the higher percentages of R cases, we find that, although, as has already been noted, a fifth of the cases fall above the chance-limit for the aggregate results, only one case exceeds its own chance-limit: that of Reagent 7, (Number of experiments, 24; per cent R, 54; chance-limit, 25; Probable per cent R + chance-limit, 35.9). This is positive evidence of an extra-chance cause, although it is greatly inferior to the evidence in Series I in which not only the aggregate results positively identify it, but of the individual cases, nine, or a fourth, at least exceed in R cases their own chance-limits.

Division II. With the Wundtian Tachistoscope.

In the second division of our experiments a tachistoscope of the Wundtian type was used. It held the white cards, 10.3 × 15.2 cm., on a hinged receiver back of a falling screen the movement of which was concealed by a larger stationary screen. Both screens were made of black pasteboard. A capital or digit, 2.5 mm. high, was typed in Elite type, with a Remington Standard Typewriter No. 7, on the center of each card. From the reagent's position, at a distance of 2 meters from the card, the letter subtended a visual angle of 4.32′. The conditions of perception were such that the reagents all together perceived about 11% of the characters, although three of the seventeen perceived none and one perceived about 30%. The ten characters used were *B H K U Z 2 4 5 7 9*.[40] The order of the presentation, determined by modified chance, provided for the exposure of each character five times in each half of a set of 100 experiments.

Procedure.

The large black screen concealed the experimenter and his manipulation of the apparatus from the reagent's view. The cards were placed on the hinged receiver in the back of the tachistoscope. After the falling screen was drawn up and the card was in place, a point of fixation on the screen held the eyes of the reagent upon the position of the character behind the screen. A signal was given 2 seconds before the exposure. The reagent recorded the character, if it was perceived, and underscored his record twice; if he did not perceive the character, but perceived lines or curves or anything serviceable for limiting his range of error in guessing, he recorded his inference or guess, and underscored his record once; if his judgment was a pure guess, he left his record without underscores. In Series I the reagent understood that any of the 26 letters or of the 10 digits was likely to appear, making his chance for R cases 2.8% (1 : 36); in Series II, he was given a list of the characters to which he confined his guesses, making chance 10%. Reagents *5*, *6*, and *7*; *12*, *13*, and *14*; and *15* and *16*, worked in groups; the rest worked alone. The rate of experiments was about one a minute; sittings as before came weekly.

[40] The following is a facsimile (natural size) of the characters:

B H K U Z 2 4 5 7 9

Fig. 3. Characters used in the Wundtian Tachistoscope.

Results.

The results of Series I, tabulated in the same manner as before, are shown in the following table:

TABLE LXV.

Wundtian Tachistoscope. $p = 2.8\%$.

	Reagent	Perceptions		Inferences		Guesses			Experiments
		No.	R	No.	R	No.	R	% R	
1914-15	*1*	..	..	16	6	84	10	11.9	100
	2	..	..	28	11	72	9	12.5	100
	4	..	..	..	..	100	2	2.0	100
	5	5	0	9	2	136	11	7.4	150
	6	..	..	5	2	145	20	13.8	150
	8	8	6	45	6	47	2	4.3	100
	9	7	2	58	5	35	0	0	100
	10	..	..	20	3	80	4	5.0	100
	11	4	2	31	6	65	7	10.8	100
1915-16	*12*	54	31	77	18	119	12	10.1	250
	13	71	25	57	7	122	10	8.2	250
	14	33	21	127	24	90	10	11.1	250
	15	58	22	70	0	72	2	2.8	200
	16	49	27	48	8	103	6	5.8	200
	17	..	..	20	2	80	0	0	100
Total		289	136	611	100	1350	105		2250
%			47.0		16.3		7.8		

Of the 2250 experiments, 289 yielded perceptions, 611 inferences, and 1350 guesses; 7.8% of the latter were Right with respect to the particular character, and 55.7% as to the general character, of the stimulus, as against the probable per cents of chance of 2.8% and 50% respectively. The distribution of the per cents R as to the particular character indicated that about half of them include extra-chance causes:

0	2	6	10	11
0	2	7	10	12
2	4	9	10	20

Series II. The results of Series II are given in the following table:

TABLE LXVI.

Wundtian Tachistoscope. $p = 10\%$.

	Reagent	Perceptions		Inferences		Guesses			Experiments
		No.	R	No.	R	No.	R	% R	
1914-15	*1*	5	3	9	2	86	9	10.5	100
	2	..	..	24	5	76	9	11.9	100
	3	..	..	..	..	100	8	8.0	100
	5	12	7	19	9	69	10	14.5	100
	6	2	1	10	4	88	20	22.7	100
	7	2	1	9	3	59	5	8.5	70
	8	6	5	36	14	58	7	12.1	100
	9	16	8	55	12	29	4	13.8	100
1915-16	17	4	4	6	2	90	24	26.6	100
Totals		47	29	168	51	655	96		770
%.....			61.7		30.3		**14.6**		

Of the 770 records, 47 were perceptions, 168 inferences, and 655 guesses; of the latter 14.6% were Right with respect to the particular character, and 55.7% with respect to the general character, of the stimulus, as against the probable per cents of 10% and 50% respectively. All but two of the per cents in the tabulation are indistinguishable from chance.

Refining our method of inspection of the results, as before, by the use of our formula, we find the values, for the two series of experiments, given in the following table:

TABLE LXVII.

Summary of Wundtian Tachistoscope Results.[41]

		Particular Character					General Character				
	Guesses	% R	p	x	L	x/L	% R	p	y	L	y/L
Series I 2250		7.8	2.8	4.94	1.8	2.7	55.7	50	6.3	5.78	1.1
Series II 770		14.6	10	4.32	4.98	0.87	55.7	50	7.1	8.28	0.86

In Series I, there was a definite extra-chance cause working for R particular guesses amounting to 2.7 times the chance-limit; for R general guesses amounting to 1.1 times the chance-limit. In Series II, since the chance-limit was only approached by 87% and 86% respectively, there is no certain indication of an extra-chance cause in the aggregate

[41] For the meaning of the rubrics, see similar table on page 196, *supra*.

results. The two individual cases, however, to which reference was made above, safely exceed their own chance-limits and give positive evidence of an extra-chance influence upon the guessing of the particular characters.

This completes our investigations of the influence of subliminal impression upon judgments given upon tachistoscopic presentations to foveal vision, and it is opportune to canvass the situation with a view of interpreting the nature of the extra-chance influence which is indicated by our tables.

From Tables LXIV and LXVII [42] we note that satisfactory evidence of this influence upon the judgments of the particular character is limited to the first series of each division of the experiments, in which the probable per cent for R cases by chance was 2.8%. Since in these summaries of our aggregate results no good evidence of the influence was shown in the second series of both divisions, in which the apparatus and procedure, and even the greater number of the reagents, were identical with those of the first series, the query may arise as to whether the extra-chance influence positively identified is in any way related to the respect in which the first series differed from the second series: That is, to the condition of guessing in which the reagent is not cognizant of the particular symbols being presented, and is not expected to limit his range of error in any degree to them. Now, as is shown elsewhere, in the section on The Influence of Mental Habits Upon Judgment,[43] the field of error is curiously limited in a reagent's guessing; the limitation is involuntarily self-imposed. Since these limitations vary with the individual, unless there is a common influence upon them, they have no effect, in the long run, upon the per cent of R cases.[43a] Were they subject to the influence of subliminal impression they would contribute to R cases, and in two ways: (1) by canceling error in the direct subliminal perception of the particular character; and (2) by limiting the range of error, in some degree, to the symbols exerting the influence and thus increasing the chances for R cases apart from either the immediate subliminal perception or pure chance. This second form of augmenting the R cases of chance, however, may also result, and perhaps more effectively, from occasional satisfactory perception of the

[42] *Supra,* pp. 196 and 200.

[43] *Vide, infra,* pp. 308 ff.

[43a] For a consideration of the influence of mental habit in these experiments, *vide, infra,* pp. 308 ff.

symbols. Is it likely, then, that our extra-chance influence for R cases is merely the effect, through suggestion, of perceptions (with their twice-underscored records) upon the range of error in guessing by narrowing the range somewhat in the more frequent guessing of the symbols that had been perceived? If so, the per cents of R cases for the individual reagents should form a sliding scale, with those who perceived no symbols at the zero end and those who perceived the greatest per cent of symbols at the other. Or, if the two arrays do not correspond closely there should be a considerable correlation between them. We find that the coefficient of correlation (r)* is

$$r = .484$$
$$PE = .077,$$

which is a definite indication of relationship; but the results of eight reagents (*9*, *17*, *22*, *25*, *26*, *27*, *31*, and *32*), in Table LXII, at the higher end of the arrays are responsible for almost the whole of this value, since if they are disregarded the coefficient of correlation between the per cent of R guesses and the per cent of perceptions becomes negligible in amount ($r = .115 \pm .109$). Results in Table LXV are clear of this criticism, as are the remaining results of Table LXII, which, after the elimination of the eight reagents, yield in R guesses 6.88%; $x = 4.1$, and $x/L = 2.08$.

Moreover, with respect to the eight reagents, the coefficient of correlation does not inform us whether the causal relation must be read from one phenomenon to the other or whether both phenomena are to some extent the effects of a common cause. It is possible that part of the relationship is the result of the influence of subliminal impression itself, since there is some evidence that unperceived letters presented by a tachistoscope facilitate the perception of the same letters exhibited in succeeding exposures.[44]

Another fact which mitigates this criticism may be pointed out. The criticism takes for its *point d'appui* the difference between the decisive x/L values of the first series and the indecisive correspond-

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$$*\rho = 1 - \frac{6\Sigma(\delta^2)}{n(n^2-1)}; \quad PE = .7063\frac{1-r^2}{\sqrt{n}}; \quad r = 2\sin\left(\frac{\pi}{6}\rho\right).$$

Vide, Brown: The Essentials of Mental Measurement. Cambridge, 1911, pp. 49 ff.

[44] Coover: Formal Discipline from the Standpoint of Experimental Psychology, *Psychological Monographs*, 1916, No. 87:203. *Cf.*, Schumann: Die Erkennung von Buchstaben und Worten bei momentaren Beleuchtung, *Bericht u. d. I Kongress f. Exp. Psychol.*, 1904:36; and Pillsbury: A study in Apperception, *Am. Jr. Psychol.*, 1896-97, 8:355, 357, 359.

ing values of the second series (Tables LXIV and LXVII). The first series has two statistical advantages over the second series, which together are probably sufficient to account for the respective differences between the values in question: (1) The greater number of experiments decreases the chance-limit (L); (2) the probable per cent of R cases by chance (p) is small enough relatively to require only about one-eleventh of the number of experiments to yield a decisive x/L value for any given per cent of extra-chance influence. The indecisiveness of the values of the second series, therefore, cannot be taken as a positive indication of the absence of extra-chance influence.[44a] As has already been pointed out some of the individual values in the other tables (LXIII and LXVI) are sufficiently high to overcome this handicap and thus to constitute positive evidence of the presence of extra-chance influence.

A second source of error, one likely to be pressed by the experimental psychologist, is the lack of a guarantee that all of the guesses are valid guesses. Is it not probable that some of the records left without underscore should have been underscored, since the reagents were not experienced in the laboratory and could not be expected to be skilled in avoiding occasional lapses of attention or memory, which naturally result in a record of a guess for a perception or a partial perception, or in avoiding momentary inexpertness in introspection? No doubt some of the guesses are not valid guesses, but they are probably negligible in number since the reagent was constantly having to discriminate between his perceptions, his partial perceptions, and his "spontaneous guesses," and, granting him customary integrity, this process itself trains to expertness in the capacity in question, and tends also to inhibit lapses of attention and memory in the critical moments of the experiment.

A consideration of the individual values in Tables LXII and LXV[45] makes it clear (1) that for about half of the reagents there is little or no extra-chance influence upon their guessing; (2) that for only about a sixth of those who made the largest percentages of R guesses, the influence of perceived letters may have been considerable; this is verified by the fact that with their elimination from the arrays, the coefficient of correlation between per cent of R guesses and per cent of perceptions falls almost to its probable error; (3) that for about a third of

[44a] The indecisiveness of the results of the second series is removed if the data are aggregated: Number of guesses = 1114, R cases = 156 = 14.5%, x = 5%, x/L = 1.25. The deviation of R cases from probability exceeds the limit of chance deviation, and the x value is 1.25 times that limit. There can be no doubt of an extra-chance cause working for R cases.

[45] *Supra*, pp. 193 and 199.

the reagents, an extra-chance influence free from any considerable error may be attributed to subliminal impression.

With respect to the R guesses of the general character of the stimulus (letter for letter, digit for digit), the y values in Tables LXIV and LXVII [46] exceed the chance-limit in two of the four cases: In Series II with the Wirthian tachistoscope, and in Series I with the Wundtian tachistoscope. Both series of characters, however, present characteristic differences between capitals and digits, as may be seen from the facsimiles of the type,[47] which may be all that is necessary to provide to indistinct perception a clue sufficiently effective to account for the excess in R cases. The process, of course, could be involuntary. Should this explanation not be adequate, there is still the possibility of subliminal influence from these distinguishing characteristics, before resort to subliminal classification of subliminally perceived particular symbols becomes necessary.

[46] *Supra,* pp. 196 and 200.

[47] Shown on p. 191, footnote 38, and p. 198, footnote 40, respectively.

Division III. Peripheral Impression.

The experiments of this division ran *pari passu* with those of the foregoing divisions, being performed with the same apparatus and general procedure as were employed in Division II. Their purpose was to present stimuli to peripheral vision under such conditions as would preclude ordinary complete or partial perception either in foveal or in peripheral vision, and thus to provide opportunity for evidence for the influence of subliminal impressions which would be free from the criticism that introspective check, upon guessing, especially with naïve reagents such as ours, is unreliable for the purpose of discriminating between guesses and partial perceptions.

On each card presented in the Wundtian tachistoscope two characters were typed in Elite type by a Remington Standard No. 7 Typewriter: a capital on the lower right corner; a digit on the upper left corner. The symbols were the same as were used in Division II: *B H K U Z* 2 4 5 7 9.* The reagent was required in each experiment to record the capital and to put down with the record any digit that should come to his mind. He was warned not to use any method in his guessing of digits. In Section I he usually did not know a digit was being presented with the letter. In Section II he did know that a digit was being presented, and he usually confined his guesses to the five digits. Conditions of perception were sufficiently difficult to demand effort, and accurate judgment of the capital remained the principal task of the reagent.

The position of the reagent was 1 meter from the card, the visual angle of the character at that distance was 8.6′, and the time of exposure was .085 seconds—less than the fixation reaction-time of the eye. Only one exposure was allowed in an experiment.

These conditions guaranteed a subliminal impression to peripheral vision, since the visual angle subtended by the distance between the two characters was 8° (in Series I of each Section), at which distance from the line of sight in peripheral vision a character to be cognized must subtend a visual angle of 3° 46′,[48]—26 times the vertical diameter of our digit,—and two strokes can be distinguished from one stroke when the distance between them is 5′ 46″,[49] which is about 67% of the angle subtended by our digit.

* A facsimile of the characters is shown on page 198, footnote 40.

[48] Volkmann: *Handwörterbuch der Physiologie,* 1846, III, 1, 334.

[49] Hueck: *Müller's Archiv,* 1840, p. 93.

In order to come closer to the limen of perception the digit for some series was brought closer to the capital: Series II, 6°; Series III, 4°; and Series IV, 2°.

In all series a fixation point on the falling screen directed the focus of the reagent's eyes upon the position of the capital.

Results.

Section I. Table LXVIII gives the data obtained under the conditions that the reagent did not know the particular digits which were being displayed with the capital, usually did not know a digit was being displayed at all, and the distance of the digit from the line of sight was 8°; the theoretical probability for R cases was 10%. The reagent, the number of experiments, the number of capitals correctly recorded, the number and per cent of digits correctly guessed, the revised chance (p_1) as determined by the restriction in the range of error which the mental habit of the reagent imposed upon his guessing, and the deviation of % R from p_1, are the respective rubrics.

Out of 2625 records of capitals, 2364 (90%) were right, and these alone are accepted as valid for our purpose. Guesses on the digits were right 365 times, making R cases 15.44%. Accepting the probable per cent for R cases (10%) as a basis for calculating x (the value of extra-chance influence), we get

$$x + p\,(1 - x) = .1544$$
$$x + .1 - .1x = .1544$$
$$x - .1x = .1544 - .1 = .0544$$
$$.9x = .0544$$
$$x = .0604 = 6.04\%$$

$L = 2.62\%$ $\qquad \frac{x}{L} = 2.3$

Since x is 2.3 times the limit of chance, the evidence for extra-chance influence is positive and satisfactory. Suspecting, however, that the relatively large per cents of R cases contributed by Reagents *23* and *27* may be responsible alone for the excess over the limit of chance variation, we disregard them and calculate our values again, finding

$$x = 4.11$$
$$\frac{x}{L} = 1.5$$

The evidence is unchanged and there appears no good reason for excluding the two records.

TABLE LXVIII.

Peripheral Vision.

Sec. I. Series I. Visual Angle 8°. $p = 10\%$.

	Reagent	Experiments	Letter R	Digit R	% R	p_1	% $R-p_1$
1914-15	*3*	100	91	9	9.5	10.0	−0.5
	4	100	97	11	11.3	11.1	+0.2
	5	99	61	16	26.2	10.0	+16.2
	6	97	71	14	19.7	11.1	+8.6
	7	100	82	12	14.6	11.1	+3.5
	8	100	80	9	11.3	11.4	−0.1
	9	99	86	10	11.6	10.0	+1.6
	10	100	96	11	11.5	11.1	+0.4
	11	100	100	14	14.0	10.0	+4.0
1915-16	*12*	100	98	11	11.2	10.0	+1.2
	13	100	100	15	15.0	11.1	+3.9
	14	100	99	10	10.1	10.0	+0.1
	15	100	93	17	18.3	11.1	+7.2
	16	100	100	14	14.0	11.1	+2.9
	17	100	85	14	16.5	11.1	+5.4
	18	100	95	17	17.9	11.1	+6.8
	19	200	189	21	11.1	10.6	+0.5
	20	100	97	12	12.4	11.1	+1.3
	21	100	94	11	11.7	8.9	+2.8
	22	100	98	12	12.2	10.0	+2.2
	23	100	98	36	36.7	11.1	+25.6
	24	100	44	5	11.4	6.7	+4.7
	25	100	96	15	15.6	12.5	+3.1
	26	100	93	14	15.0	11.1	+3.9
	27	100	98	32	32.6	13.3	+19.3
	28	30	23	3	13.0	11.1	+1.9
Totals		2625	2364	365			
%			90	15.44			

The extra-chance influence might result from common mental habits in guessing, such as by narrowing the range of error would increase the chances for R cases. Upon examination of tabulations of digits guessed, it is found that but 7 reagents out of the 26 were free from those self-imposed limitations. Usually they neglected to guess *0* or *1*, or both, which would increase the chance for R cases from 1 : 10 to 1 : 9, or 1 : 8; but sometimes an intermediate digit was ignored and occasionally it was one of the digits being displayed. For each reagent, then, his chance for R cases, as affected by his mental habit, was found, and is given under p_1 in the table above. If we segregate the data and calculate x

on the basis of the revised values we get the following table, in which $p_1 n$ is the probable number of R cases expected from the chance given under p_1:

TABLE LXIX.

Mental Habit Eliminated.

Experiments	p_1	$p_1 n$	R
1130	11.1	125.4	190
633	10.0	63.3	82
189	10.6	20.0	21
98	13.3	13.0	32
96	12.5	12.0	15
94	8.9	8.4	11
80	11.4	9.1	9
44	6.7	2.9	5
2364		254.1	365
Per cent		10.75	15.44
Difference			+4.69

$$x + .1075 - .1075x = .1544$$
$$x - .1075x = .1544 - .1075 = .0469$$
$$.8925x = .0469$$
$$x = .05255 = 5.25\%$$
$$L = 2.62\% \qquad \frac{x}{L} = 2.02$$

The aggregate of the R cases expected in the 2364 experiments becomes 10.75% instead of 10%; the difference is real but slight, and x is still twice the chance-limit. The extra-chance influence, after the effect of mental habit is eliminated, seems to be due alone to the subliminal impression of the digit made upon the peripheral retina. But it is not equally effective with all of the reagents. From the last column in Table LXVIII we get the following distribution of gross values of extra-chance influence (%R–p_1):

-0.5	+1.9	+4.7
-0.1	+2.2	+5.4
-0.1	+2.8	+6.8
+0.2	+2.9	+7.2
+0.4	+3.1	+8.6
+0.5	+3.5	+16.2
+1.2	+3.9	+19.3
+1.3	+3.9	+25.6
+1.6	+4.0	

Since the chance-limit for the distribution is about 2.6, we may infer that the subliminal impression was effective in 15 of the 26 cases.

The results of the further series, given for the purpose of learning whether the extra-chance influence becomes greater as the visual angle between digit and letter is decreased toward the limen of peripheral vision, are summarized in the following table:

TABLE LXX.

Summary of Section I. $p = 10\%$.

Series	I	II	III	IV
Visual angle	8°	6°	4°	2°
Number of reagents	26	15	14	14
Number of experiments	2625	760	410	280
Letter R	2364	721	403	276
% Digit R	15.44	12.07	12.65	21.76
p_1	10.75	10.54	10.81	12.36
x	5.25	1.71	2.03	10.7
x/L	2.02	0.36	0.32	1.27

These reagents, it must be recalled, did not know what digits were being presented, and, owing to mental habits, variously restricted their range of error in guessing; this source of error has been removed from their results in the above table. Revised chance (p_1) always exceeded theoretical p somewhat, and in Series IV considerably. The x value falls heavily in Series II, but gradually increases in the succeeding series until it becomes (in Series IV) 1.27 times the chance-limit, owing largely, no doubt, to direct peripheral perception. The drop was unexpected, but seems to be due to a strong counter-tendency which the subliminal impression had overcome. Tabulation of the digits guessed shows that associations were formed between letter and digit, sometimes strong enough to persist involuntarily in the reagent's guessing for a considerable time. In fact, since subliminal impressions must have been to some extent responsible for these associations, the longer the reagent was kept guessing, under the same conditions, the more firm would the associations become by reason of continued subliminal reinforcement; and, apart from reinforcement, the process of recording the associated symbols together would tend by the force of habit to make the associations firmer. Now, in each Section, the reagents who took the successive series of experiments were the same reagents, and they took the series in their consecutive order. But for each succeeding series a fresh set of cards, with a rearranged distribution of the digits over the capitals, was provided, and continued associations, in so far as they were the result of subliminal impression, would result in 100% wrong guesses.

Their influence would have to be more than overcome, for R cases to exceed chance, by the subliminal influence of the newly distributed digits. The amount of decrease in the x value, in Series II, might then be taken as a rough measure of the influence of persisting associations.

Although some of the x values (in Series II and III) are not greater than their associated chance-limits, probably owing wholly to the lack of a sufficient number of experiments to reduce the chance-limit below them, they indicate gradation of influence with successive decreases in the visual angle between the digit and the line of vision. That the gradation indicates merely decrease in the counter-influence is unlikely, since the series II, III, and IV were taken in the same sitting and the number of experiments in each was scarcely enough, except through the influence of subliminal impression, to break up the persisting associations. At any rate, the tabulations above referred to show that some of the associations persisted to the end.

With respect to the x values of the last two series (III and IV), it may be fairly questioned whether they are free from direct peripheral perception. The following is testimony from the reagents' introspections:

Reagent *16*: Saw no numbers or spots.

Reagent *18*: Did not know there were any digits on cards; did not see spots.

Reagent *19*: Without knowledge of digits or spots throughout.

Reagent *21*: Did not know a digit was shown.

Reagent *26*: Knew digit was on card, but did not know which digits; did not know they grew closer in Series II-IV.

Reagent *27*: Did not see any digits or spots, but learned that digits were on cards, somewhere about half way through Series I.

Reagent *20*: Knew digits were there but did not know what ones. But after first few series did not notice the dark spot in corner [of card]; did not know that in Series II-IV the dark spot moved closer.

Definite report is available from only about half of the reagents, but no reagents made a greater number of R cases in the respective series than some of the reagents quoted above. With respect to the visibility of the digit in Series III and IV, we shall have to rely upon the results of the reagents in Section II who knew that they were there and tried to see them. It is possible that direct perception played some part in the x value (10.7%) of Series IV, but, as implied above, the advantage is not distinguishable in amount from the effect of subliminal impression on the guessing of the reagents who perceived no digits.

Section II. In Section II, although there were not as many experiments carried out as would be necessary to acquire scientific proof of the influence of subliminal impression, there is good support for Section I in indicating a gradation of influence as the digit approached foveal vision. The data are summarized in the following table:

TABLE LXXI.

Summary of Section II. $p = 20\%$.

Series	I	II	III	IV
Visual angle	8°	6°	4°	2°
Number of reagents	9	9	9	10
Number of experiments	270	270	180	200
Number of Letter R	240	261	175	195
% Digit R	21.7	24.2	28.0	47.2
x	2.13	5.25	10.0	34.0
x/L	0.20	0.50	0.70	2.80

The extra-chance influence (x) in the last series (IV) is 2.8 times the chance-limit; valid peripheral perception having played a greater part than in the preceding Section, since these reagents knew not only what digits were displayed, but where they were displayed, and directed their attention accordingly. Since the extra-chance influence was not more than 34%, the conditions of perception were still below the conventional limen (50%) of perception. This fact, in connection with the indications in the last two tables, of a gradation of influence corresponding inversely to the change in angular distance between digit and line of vision, emphasizes the conclusiveness of the results shown in Table LXVIII, in their demonstration of the influence of subliminal impression.

Introspective report of the reagents who knew where the digits were and endeavored to see them, supports the assumption that the 8° peripheral impression was safely beyond the limits of peripheral perception:

Reagent *39*: Saw the spot in Series IV (2°) but am not sure of any [guesses of] digits. (R cases = 45%).

Reagent *37*: Saw digits in Series IV only. (R = 35%).

Reagent *34*: Could see digit occasionally in Series III (4°) (R = 15%), and more often in Series IV (2°) (R = 45%).

Reagent *33*: Perceived digits [spots] in latter part of Series III (4°), but I could not tell what they were; could tell only that they were there. (R = 15%; in Series IV, R = 25%).

Reagent *30*: In first experiment of Series IV saw both the *B* and the *4*

(R = 85%; in Series III (4°), R = 50%; in Series II (6°), 33%; in I (8°), 17%—chance in all 20%).

Reagent *31*: Was able to see most of the digits in Series IV (2°) (R = 85%; in Series III (4°), R = 35%).

Reagent *35*: In Series III and IV I could see that the digits were in a different spot, that they were coming nearer to the letter; I could see them but I could not make them out. I thought I saw a digit once in the last series (IV, 2°, R = 30%).

Hence, under the conditions of full knowledge of the position of the digit and an inclination to see it if possible, some reagents can occasionally see the digit in Series IV (angular distance 2°) and rarely in Series III (angular distance 4°). The values of x in the last table may, consequently, be inferred to be constituted largely of the influence of subliminal impression. The table supports the deduction above concerning the counter-influence of associations, since, owing to the smaller number of experiments (30) made by a single reagent in Series I, there is no drop in the x value of Series II, as was the case in Section I, where the reagent made 100 experiments in Series I.

Before leaving this division of our investigation, two special incidents connected with it should perhaps be reported.

(1) It was customary for two students to work together, one filling the office of experimenter, the other that of reagent. Thus the first reagent could be kept free from knowledge about the digit, and his results contributed to Section I; the second reagent had acted as experimenter, knew about the digits, and confined his guesses to the five digits presented; but since a new set of cards was used for him with a new distribution of digits over the capitals, he was instructed to free himself from any associations he had made during his performance as experimenter. His results contributed to Section II. One second reagent, however, through error was given the same set of cards in her first series that she had handled as experimenter, and guessing, being controlled by associations she must have made, resulted in 28 R cases out of 29 experiments in which the letter was correctly recorded. Before learning of the results, she wrote the following introspection: "I knew that with every letter there was a digit and that each letter had the same digit every time, but I did not know what the digit was for each letter." Three explanations offer themselves: (1) She had unconsciously formed the associations while handling the cards as experimenter; (2) she was extremely susceptible to the subliminal influence and formed the associations immediately upon the presentation of the cards to her as reagent; or (3) her introspection is unreliable. Since she seemed to fully

realize the responsibility of assisting in a research the results of which were expected to be published, and since her associations persisted in succeeding series in opposition to subliminal impressions of different digits, the first explanation seems the most satisfactory, and, if true, implies a remarkable performance in unconscious association. Her results, of course, were not used in the tables.[50]

(2) Reagent *23* had been found to be a fairly ready crystal-gazer, and he was provided with a glass paper-weight, topped with a spheroidal knob about 3.5 cm. in diameter, into which he looked for his digits, thus dispensing with direct guessing. He was not to influence the digit in any way, but to record the digit that appeared. His results were higher in R cases than the average, as may be seen, for Series I, from Table LXVIII;[51] per cents of R cases in Series II-IV being 10.3, 27.6, and 70.6 respectively. Concerning the process, he said that part of the letter, always projected into the crystal normal size, suggested the form of the digit, which came immediately and appeared objective and definite. But since the digits recorded with the letters changed freely from series to series, the suggestion was evidently not provided by, or confined to, the form of the exhibited capital. The crystal, apparently, served as a device for converting the subliminal impressions into hallucinatory perceptions.

[50] The reagent has read this paragraph in the manuscript and affirms that it should not be crossed out; that the inference concerning the "unconscious association" must be correct.

[51] *Supra*, p. 207.

Division IV. Miscellaneous Series.

The miscellaneous series of experiments reported in Division IV are to be regarded as supplementary to those reported in the foregoing divisions. Although they are fewer in number, were performed under less rigorous experimental control, and, consequently, are of a lower order of proof for the influence of subliminal impression upon judgment, they are in some cases sufficiently positive to warrant their record; and their inclusion in our report is essential to the ideal of reporting all the facts, a departure from which would expose the report to the criticism that only selected results were displayed.

The stimuli presented and the method of experiment were suggested by work reported in psychical research literature. The following tabulation will show in outline the nature and number of the experiments in each of the four sections:

Subliminal Impression from	Number of Reagents	Number of Experiments
§ I. Corneal reflections of playing-cards	2	400
§ II. Playing-cards viewed at a distance	7	900
§ III. Whispering of the stimulus—		
1. Whispering of letters and digits	4	800
2. 1st Series. Whispering of playing-cards	2	200
2d " " " " "	2	400
3. Whispering of numbers	1	60
§ IV. Involuntary signs (number-guessing)	1	60
Totals	19	2820

§ I. Subliminal Impressions from Corneal Reflections.

Reflections from the cornea of the eye, it is often said,[52] may play an important rôle in feats of mind-reading and even in experiments in thought-transference when the percipient faces the agent. They are definite, minute, fleeting, elusive; possible stimuli for testing the influence of subliminal impression. Consequently two students took turns in acting as experimenter and reagent, performing two sets of 100 experiments each in card-guessing. Conditions were made favorable for seeing the reflection. The experimenter sat with his back to the light (his right eye in shadow), and when he drew a card he held it up about four or five inches from his face and slightly to the right side, in good light.

[52] *Vide*, Bergson: *Revue Philosophique*, 1886, 22: 531.

The reagent watched for the corneal reflection at a distance of about two feet. Judgment was given separately upon color, number, and suit, and perceptions were underscored; guesses were left without underscore. The conditions of perception may be seen from the following table:

TABLE LXXII.

Underscored Records.

Set	Color		Number		Suit		Experiments
	Total	R	Total	R	Total	R	
I	50	48	91	81	6	4	100
II	100	99	45	43	7	7	100
III	98	95	50	46	15	13	100
IV	9	8	95	76	0	0	100

It is seen that for one reagent (Sets I and IV) the number of spots, and for the other (Sets II and III) the color, is the clearest feature of the reflected image; suit is particularly difficult to determine.

The success of the guesses is shown in the following table:

TABLE LXXIII.

Corneal Reflection. Guesses.

Set	Color			Number			Suit		
	No.	R	% R	No.	R	% R	No.	R	% R
I	50	41	80	9	2	22	94	57	61
II				55	30	55	93	50	54
III	2	1	..	50	26	52	85	40	47
IV	91	79	87	5	2	..	100	56	56
Totals	143	121		119	60		372	203	
%		84.6			50.5			54.6	
p%		50			10			40.8	
Dev.		+34.6			+40.5			+13.8	

The deviations are sufficiently large to make the presence of an extra-chance influence certain. On number, however, it is certainly augmented by reflections of spots of such a character that they afforded a basis for approximation, and chance would not be merely 10%; nor is there any way of estimating it. And chance on suit is estimated on the basis of 257 underscores for color. Of course, when the color is known p for suit is 50%; but if color is not known it is 25%.

$$\left(p = \frac{(.50 + 257) + (.25 \times 143)}{400} = .408.\right)$$

Since the suit is the safest stimulus for testing for the influence of subliminal impression, it merits more thorough consideration. A retabulation gives the following table:

TABLE LXXIV.

Guesses on Suit.

	Color Underscored				Color Not Underscored			
Set	No.	R	% R	p	No.	R	% R	p
I	47	27	57	50	47	30	64	25
II	93	50	53.7	50				
III	83	40	48	50				
IV	9	5	55.5	50	91	51	66	25
Totals	232	122			138	81		
%		52.5				58.7		
p		50				25		
Dev.		+2.5				+33.7		
ε		5%				45%		
x/L		0.36				2.9		

When the corneal reflection was clear enough for the color to be perceived, the deviation of R cases from the probable per cent on suit (+2.5%) is not large enough to demand attention; the corresponding x value being only 5%, which is but 36% of the chance-limit.

When the reflection, however, was not clear enough for the color to be perceived, and the probable per cent of R cases due to chance was 25%, the deviation of R cases is large and significant (+33.7); the corresponding x value being 2.9 times the chance-limit, which for so short a series is very large.

We have, then, two significant extra-chance values: 84.6% on color (Table LXXIII), and 58.7% on suit (Table LXXIV); the x value for the former is 68.2%, and is 4.06 times the chance-limit. Both values are the results of the work of but one of the reagents (Sets I and IV). There is no satisfactory way to determine how much error, owing, say, to inexpert introspection in underscoring, is included in these values. But in the light of the results with the peripheral digit guessing, it would be not unreasonable to accept these values as indicative of considerable subliminal influence from the corneal reflection of spots on playing-cards.

§ II. SUBLIMINAL IMPRESSIONS OF PLAYING-CARDS AT A DISTANCE.

Both Sidis and Stroh, Shaw, and Washburn[53] used traced or printed characters (letters, or letters and digits) for stimuli, and made the impression subliminal by placing the stimulus so far away from the reagent that it could not be perceived. The uniformity of spots in a pack of playing-cards (court cards eliminated) suggested their use in a similar way. Especially, the forms of the spots (suit), which in the experiments with corneal reflection proved to be excellent stimuli, were expected to provide subliminal impressions the influence of which could be decisively demonstrated.

Consequently, 900 experiments were carried out with seven reagents. Each characteristic of the stimulus, Color, Number of spots, and Suit, was judged separately; and if perceived the record was underscored. A linen-finish card[54] with very clear, well-printed spots was used. Reagents *I-IV, VI,* and *VII* were stationed 32 meters from the card; Reagents *II* and *V,* 25 meters; and Reagent *III,* 10 meters. The following table gives the gross results:

TABLE LXXV.

Guessing of Cards at a Distance.

	Underscored						Not Underscored					
Reagent	Color		Number		Suit		Color		Number		Suit	
	No.	R	No.	R	No.	R	No.	R	No.	R	No.	R
1	100	99	96	79	97	66	0	0	4	0	3	1
2	54	49	2	1	4	2	46	33	98	35	96	39
3	54	53	53	51	0	0	46	45	47	38	100	38
	94	94	89	88	0	0	6	6	11	8	100	67
	88	88	89	87	0	0	4	3	3	0	92	54
4	74	62	22	17	0	0	26	22	78	46	100	47
5	28	23	1	1	1	1	72	45	99	12	99	33
6	67	59	49	49	0	0	33	24	51	32	100	43
7	99	95	80	64	29	19	1	0	20	14	71	31
	658	622	481	437	131	88	234	178	411	185	761	353
% R		94.5		90.8		67.2		76.0		45.0		46.4
p								50		10		41.4
Dev.								+16		+35		+5.0
x								32				8.5
x/L								2.3				1.28

[53] *Vide,* pp. 177 ff., and 186 ff., *supra.*

[54] Aristocrat, linen finish, Kalamazoo Playing Card Company.

The left half of the table indicates, by the amount of underscoring, and the accuracy of the underscored records, the general conditions of perception. The remaining part of the table gives the guesses, upon which the amount of influence of subliminal impression must be determined. The deviations from the probable per cent of R cases are: Color +16, Number +35, Suit +5. And since Number may be approximated by the apparent fullness of the card or by the pattern of the spots, the corresponding deviation must be disregarded. On Color the amount of extra-chance influence (x) is 32%, which is 2.3 times the chance-limit; on Suit it is 8.5%, 1.28 times the chance-limit. p for Suit, however, was calculated,[55] and our knowledge would be more satisfactory if the guesses on the Suit were segregated and the respective deviations from the two probabilities were used separately for determining x values. The segregated data are shown in

TABLE LXXVI.

Guesses on Suit.

	Color Underscored			Color Not Underscored		
Reagent	No.	R	% R	No.	R	% R
2	44	24	54.5	47	15	31.9
3	(54	19	35.2)	6	5	
	88	64	72.6	6	3	
	84	51	60.7	4	3	
4	59	33	55.9	27	11	40.7
5	22	12	54.5	73	20	27.4
6	57	31	54.4	33	12	36.4
7	68	31	45.6	2	0	
Total	476	265		198	69	
%		55.7			38.8	
p		50			25	
Dev.		+5.7			+13.8	
x		11.4			18.4	
x/L		1.17			1.42	
Or Total	422	246		252	88	
%		58.3			35	
p		50			25	
Dev.		+8.3			+10	
x		16.6			13.3	
x/L		1.6			1.15	

[55] $p = \frac{(234 \times .25) + (622 \times .50)}{892} = .414 = 41.4\%$. When Color was underscored, and wrong, the chance for R cases on Suit is 0; when right, 50%; when unknown, 25%.

The x values are positive and they exceed their respective chance-limits. indicating certainly an extra-chance influence upon the guessing of the Suit both when the color was known and when it was unknown. The 35.2% R, of Reagent *III* under "Color Underscored," really belongs to the other side of the table, since the reagent, a Japanese, explained that he did not know what suits were black and what red. If it is transferred, the respective x values become 16.6% and 13.3%; x/L, 1.6 and 1.15; no essential change being made in the evidence for the influence of the subliminal impression of distant card-spots upon the guessing of Suit. Of course, this evidence stands upon the trustworthiness of the underscoring; but Table LXXV indicates that the underscoring occurred on records that must have been much below clear perception, only 67.2% of the underscored records on Suit being right. The spot, therefore, seems quite satisfactory as a stimulus to be made subliminal by distance. There is a source of error, however, arising from the conventional spot patterns of the cards which enables the reagent to infer the Suit of a fairly full card: a nine-spot or ten-spot of Hearts is a redder card than the corresponding Diamonds; "Diamonds look larger than Hearts and, if placed one above another, seem more connected at the distance of 32 meters." "Clubs look rounder than Spades, and a number of them gives a somewhat blurred effect not given by Spades," says Reagent *VI*; but Reagent *VII* said, "As to suit, this was a guess most of the time, Spades appeared to be a larger blur than Clubs; Hearts and Diamonds were a quandary; could distinguish neither correctly and had no basis to start a guess on. Sometimes I recognized a stronger blur and called that Hearts, as a card of Hearts is covered with a little more red than a card of Diamonds."

This error would seem to be largely limited to the full cards seen well enough to determine the color, and, consequently, would affect but part of Table LXXVI. If the color of the blur could not be distinguished, it is improbable that the actual differences in the size of the spots would rise above the threshold of discrimination.

§ III. THE WHISPERING OF THE STIMULUS.

An explanation offered in the early days for the successful experiments in Thought-Transference was the unconscious perception of involuntary whispering,[56] and recent experiments[57] have strongly sug-

[56] *Vide,* Hansen and Lehmann, *op. cit.*

[57] *Vide,* Stroh, Shaw, and Washburn, *op. cit.*

gested that the whispering of letters increases R cases in guessing even though the reagent hears no sound.

In order to test further the hypothesis that subliminal auditory impressions may influence judgment, the following experiments were made. The stimuli were varied: whispered names of (*a*) letters and digits, (*b*) playing-cards, and (*c*) numbers.

1. The Whispering of Letters and Digits.

Four reagents, all men, made 800 experiments in recording from whispers of letters or digits originating at a source 25 meters distant. The same letters and digits and the same general method of procedure were used as in the Tachistoscopic experiments. The whispering of the character was not begun until after trial a manner of whispering, as to intensity and articulation, was found which was judged to be just below the limen of perception.

As before, perceptions were underscored twice if the whispered letter was perceived, once if the whispering helped the guess; only spontaneous guesses were recorded without underscoring. In Series I each reagent was in ignorance of what letters or digits were being presented (chance = 2.8%); in Series II he limited his guesses to them (chance = 10%).

TABLE LXXVII.

Guesses of Whispered Letters and Digits.

Series I. $p = 2.8\%$.

Reagent	No. of Experiments	Perceptions Total	Perceptions R	Inferences Total	Inferences R	Guesses Total	Guesses R	% R
1	100	9	4	26	7	65	6	9.2
2	100	6	5	55	29	39	7	18.0
3	100	2	2	6	1	92	4	4.3
4	100			7	0	93	4	4.3
Total	400	17	11	94	37	289	21	
%			65		39.6		7.26	
p							2.8	
Dev.							+4.46	
x							4.6	
x/L							1.12	

TABLE LXXVII (*Continued*).

Series II. $p = 10\%$.

Reagent	No. of Experiments	Perceptions Total	Perceptions R	Inferences Total	Inferences R	Guesses Total	Guesses R	% R
1	100	16	11	40	24	44	4	9.1
2	100	12	10	54	29	34	5	14.7
3	100	3	3			97	12	12.4
4	100					100	12	12
Total	400	31	24	94	53	275	35	
%			77		56.4		12	
p							10	
Dev.							+2	
x							2.2	
x/L							0.29	

The deviations for the respective series are +4.46% and +2%; the x values are 4.6 and 2.2; and the x/L values, 1.12 and 0.29. Consequently, in Series I the x value, since it exceeds the limit of chance deviation, portrays definitely an extra-chance cause operating for R guesses; but in Series II the corresponding value, being only 29% of the deviation from the probable per cent which may be expected by chance, is not determinative. Since all but one of the sets in the latter series, however, show a positive deviation from the probable number of R cases, the x value of the series is probably not due to chance.

The decisive value for an extra-chance cause in Series I becomes still more satisfactory as a determination of the effect of subliminal auditory impressions upon judgment when it is recognized that the underscoring was carried out to a level considerably below the threshold of perception, only 65% of the perceptions being right, and 39.6% of the inferences. The remaining records are thus pretty certain to be spontaneous guesses, free from the error of inexpertness in introspection.

Tabulation of the wrong guesses for the purpose of learning whether they were more often right as to classification, than they should be by chance (*i. e.* letter for letter, and digit for digit), yields only chance deviations from the probable per cent. No subliminal classification has taken place.

2. The Whispering of Playing-Cards.

Two series of experiments were carried out with the intention of seeking the effect of slight whispering upon card-guessing. In Series I two reagents performed 100 experiments each under conditions analogous to those of the thought-transference experiments described above.[57a] The chief variation in method was that when the dice cast odd the name of the card was lightly whispered. In Series II all the cards drawn were lightly whispered. Two reagents performed 200 experiments each. In both series the reagent sat 32 meters distant from the experimenter, who whispered the names of the cards (as, 5 of Hearts). The following table shows the results:

TABLE LXXVIII.

Guesses of Whispered Cards. % Right.

Series I.

	Card Not Whispered					Card Whispered				
Reagent	Card	Color	No.	Suit	Total	Card	Color	No.	Suit	Total
1	4	39	16	20	49	2	39	8	18	51
2	4	46	8	17	48	4	65	13	26	52
Probability	2.5	50	10	25		2.5	50	10	25	

Series II.

1						2	45	7	25	100
"						2	62	7	29	100
2						1	47	14	24	100
"						2	55	9	32	100
Probable %						2.5	50	10	25	

The deviations of R cases from the probable per cent in both series are much too small to indicate any extra-chance influence upon the whispering. In Series II, Suit, in the last set, seems to have some advantage; but to be decisive it should be 43% instead of 32%. The whispering does not appear to have had any influence upon the guessing.

3. The Whispering of Numbers.

Remembering the success of the "Unwillkürliche Pflüstern" of

[57a] *Supra*, pp. 48 ff.

Hansen and Lehmann [58] when they conducted their guessing experiments with the aid of large parabolic mirrors at the respective foci of which the closed mouth of the experimenter and the attentive ear of the reagent were placed, we designed, for demonstration purposes primarily, a series of 60 experiments in which the student acted as experimenter and the writer as reagent. The numbers chosen for the experimenter to whisper so lightly that no movement of the lips or throat, and no sound, could be detected by an observer, were *2*, *3*, and *8*. A short pasteboard tube made communication between the left ear of the experimenter and the right ear of the reagent, slight sounds due to changes in breathing were heard through the tube at the moment of "involuntary" whispering, and R cases were 52%, 28% [59] being due to the influence of the slight sounds.

§ IV. INVOLUNTARY SIGNALS

A short series of experiments designed to demonstrate the fact of involuntary signals was carried out by two women students from one of the writer's classes.

One of the students, who had a definite "number form," that is, a spatial system over which the number-series was distributed, acted as experimenter. The other, who was confidentially instructed in the essentials of the "number form," acted as reagent and did the "guessing." The three numbers (*10*, *20*, *30*) selected for the experimenter to "think" lay in distinctly different locations in the scheme. Each student was provided with a ruled form containing 60 spaces. The experimenter closed her eyes and thought of a number, which she recorded after the reagent had recorded a guess. The latter formed her guess after watching the movement of the cornea of the eye under the experimenter's closed lids. Occasionally this movement betrayed the number thought of by locating spatially its position in the "number-form." The reagent was inexpert and skeptical of success; yet her R cases equaled 60%, 40% [60] being due to the influence of the involuntary signs. Her skepticism indicates that the assistance was mainly unconsciously used; that is, that subliminal impressions of the involuntary signals possibly influenced her judgments.

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[58] *Vide, supra,* p. 32.

[59] $x = .52 - p\,(1 - x) = 28\%$. $x/L = 1.12$.

[60] $x = .60 - p\,(1 - x) = 40\%$. $x/L = 1.8$.

Conclusion.

The influence of subliminal impressions, of capitals and digits presented by a tachistoscope to foveal vision, produced in spontaneous guessing from 5% to 7% Right cases. That this effect is only about 25% of that found by Sidis and about 17% of that found by Stroh, Shaw, and Washburn, may be owing to two radical departures in the method of experimentation: (1) The stimulus was rendered subliminal by decreasing the time of exposure rather than by increasing the distance between stimulus and reagent; and (2) the reagent was required to designate a class of records (inferences based upon partial perception) between perceptions and guesses, which proved to be a large class, and which was excluded from consideration. The influence was operative upon only about 30% of the reagents; and its effect was mainly to cause a right guess of the particular character. There was some indication that it contributed to R cases in the guessing, with respect to the general character of the stimulus (letter for letter, digit for digit), since both styles of type that were used exhibited characteristic differences between capitals and digits. For this latter effect, it does not, therefore, seem necessary to assume a subliminal perception of the particular characters,[61] or a subliminal elaboration or the classification of particular characters.

That the effective subliminal impressions cannot be regarded merely as sensations of sufficient intensity and clearness to be noticed in consciousness but unnoticed by our reagents because of misdirection of attention incident to inexpertness in introspection, is shown by the effect of digits presented to peripheral vision (visual angle 8°) during an interval less than the reaction-time of the eye. Normal perception is impossible, yet the effect was to increase right guesses by about 5%.

The results of some of our experiments indicate that the tachistoscopic control of the stimulus may be less satisfactory than the distance control for inciting the influence of subliminal impressions. Corneal reflections of the spots on playing-cards, when too indefinite for the perception of their color, exerted an extra-chance influence on R guesses of 45%; and guessing on the same stimuli from a distance too great to afford perception of their color, was influenced toward R cases to the extent of 13%.

Subliminal influence of auditory stimuli consisting of whispered letters and digits was found to the extent of about 5%, although similar

[61] *Cf.*, quotations from Sidis, pp. 182 ff., *supra*.

whispering of the names of playing-cards produced no noticeable effect upon the guesses of the few reagents tried.

In a short series of guessing in the presence of involuntary signals (movements of the eye), when the extra-chance influence amounted to 40%, there was strong indication that the assistance of the involuntary signals was largely unconsciously received.

Just how subliminal impressions are able to influence judgment must remain a matter of conjecture. No doubt the neural processes set up by the stimulus differ from their analogous processes subserving conscious perception only in intensity, the essential difference between their psychical concomitants being that the sensations accompanying the former are not discovered and recognized by introspection. This lack of recognition may result from two distinct mental conditions: (1) Abstraction upon exclusive content; as, to make use of a literary miscellany, Newton's astonishment, upon reaching home, at finding himself wet, resulted from his state of mental abstraction during the homeward journey in which his horse took a short cut and swam the mill-pond. The sensations of temperature and touch were supra-liminal in intensity, but they were disregarded,—a matter of the direction of the attention. (2) Inability to directly recognize subliminal impressions. Just as there are differences between sensations too small to be clearly recognized, as between the "hefts" of the 102- and 103-gram weights, so there must be sensations too obscure in consciousness to be clearly recognized,—a matter of "Weber's Law," or of the "Law of Relativity." The impressions are "subliminal" in the sense that they fall below the conventional limen of perception; not that they fall below the absolute limen of consciousness.

It is probable that both of these conditions obtained, perhaps unequally, in our experiments when the presentation was made to foveal vision; but only the latter could have obtained when the presentation was made to peripheral vision.

We have found, therefore, in our investigation of the effect of subliminal impression upon judgment, some experimental evidence of "a fringe of perceptions, most often unconscious, but all ready to enter into consciousness, and in fact entering in in certain exceptional cases or in certain predisposed subjects" with which Professor Bergson insisted "'psychical research' could and should concern itself."[62] And it must

[62] *Vide*, quotation, p. 170, *supra*.

be regarded as more than probable that, as some investigators and critics have suspected,[63] this sort of perception has played a rôle in the evidence for telepathy gathered from thought-transference experimentation and from the séance-room. Indeed, the argument offered, with specific reference to his own remarkable experiments in thought-transference, by Professor Gilbert Murray, in his Presidential Address on The Fringe of Consciousness (delivered in the Hall of The Royal Society of Medicine, London, July 9, 1915, before the Society for Psychical Research), goes further:

> (1) The action of what we call telepathy is habitual in ordinary human intercourse.
>
> (2) In essence it consists in nothing more mysterious than the action on the mind of sense-perceptions which are too faint to establish themselves, as sense-perceptions, above the threshold of consciousness.[64]

Further investigation in this field may profitably look toward the determination of the extent of the influence of subliminal impression upon judgment (*a*) in normal subjects, when the stimuli are not removed so far from the limen of normal perception, and when the stimuli are varied over the sense-modes, and (*b*) in 'sensitive' or 'psychic' subjects.

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[63] *Vide*, the abstracts from the literature at the beginning of this Part. *Cf.*, Thomas: Thought Transference, p. 28; Stout: *Hibbert Jr.*, 2:62.

[64] *Proceedings S.P.R.*, 1916, 29:48.

PART III.

MENTAL HABIT
AND
INDUCTIVE PROBABILITY

PART III.

MENTAL HABIT, AND INDUCTIVE PROBABILITY.

The two concepts, Mental Habit, and Inductive Probability, are so often connected in the minds of students of psychical research with the experimental investigation of thought-transference, as possible factors of error and delusion, that it has been thought worth while to give specific attention to them [1] at this point; especially since they also have an equally vital bearing upon our Experiments on Subliminal Impression.

In the first section of this Part we shall endeavor (1) to show, by way of illustrations from the most diverse sources, how inevitably mental habit influences judgment irrespective of the field in which judgment is made; (2) to examine the bearing of this general fact upon the investigation of thought-transference; and (3) to determine the amount of extra-chance influence which may reasonably be credited to this cause, in such experiments in thought-transference as have been reported in the literature, and in such experiments as we have performed on Subliminal Impression.

In the second section of this Part we shall endeavor to show, also by way of varied illustrations, that empirical chance events occur in conformance to the mathematical law of chances, and, consequently, that the "Theory of Probability" and the statistical formulæ derived from it provide us with a means which cannot fail to resolve doubt and controversy concerning the existence of the alleged phenomena of thought-transference, lucidity (clairvoyance), and communications from discarnate personalities who have the power of learning facts in our world.

In these endeavors the data of our experiments are brought under tribute, and, it is hoped, some slight but positive contribution may be made upon the general topics of both sections.

[1] Professor Titchener, as early as 1899, stated that in his opinion the next step in thought-transference investigation should be "an exhaustive study of number-habits." (*Science*, 1899, 9: 787.)

MENTAL HABIT.

THE INFLUENCE OF MENTAL HABIT UPON JUDGMENT.

The phenomena of subliminal influences upon judgment constitute a fairly large class, only one genus of which is the influence from ordinary sense impressions, which was subject to investigation in Part II, and since it is of interest to psychical research to explore thoroughly the "sub-conscious" phenomena we have an additional incentive to notice another important genus of subliminal influence, which has played a rôle in the evaluation of the results of experiments on thought-transference, and for which our data have furnished some evidence—the influence of mental habits upon judgment.

It has long been known that judgments are modified in such ways as are not creditable to the person delivering them, and thus involuntarily, or in such ways as show systematic or constant biases where no conscious motive can be conceived to operate; but only after general interest in statistical matters had been developed in the sociological and psychological fields did knowledge of the extent of this involuntary control of judgment, and of its nature, become definite. We now know that mental habits pertain to numbers, letters, colors, time and space divisions, and to almost everything about which a judgment can be made at all, and that they are so prevalent that perhaps no considerable number or class of judgments is free from them, whether the judgments are delivered by a reagent in a thought-transference experiment in a psychological laboratory, by a judge of the supreme court in sentencing a convicted prisoner, by a teacher who is estimating the scholastic merit of a student in assigning him a grade-mark in a specific subject, by an astronomer who is estimating the magnitude of a star, by a signal service meteorologist in estimating the temperature or the amount of precipitation, or by a psychologist who is estimating tenths of a vibration on a kymograph time-record.

Let us examine some of the available statistical evidence of the influence of mental habit upon judgment.

Population by Age.

In the report of the Twelfth Census of the United States (1900) we find the following statement concerning the "Round Number" tendency that people have in reporting ages:

> In every census of population . . . there is apparent a considerable concentration on certain years of age, particularly on those that are multiples of 5 and 10, and in a somewhat less degree on the even years of age for persons less than 20 years old. This concentration is due to the well known tendency to state ages in round numbers, especially in those cases where the person interrogated is not possessed of positive knowledge concerning members of the household (relatives and boarders) who are absent at the time of the enumerator's visit.[2]

Inspection of Plate XIII,[3] which gives the distribution curve of the population between the ages of 1 and 72 years, for the mainland of the United States, according to the Twelfth Census, will verify the above statement; and the round-number and even-number preferences are found to obtain for both male and female population. An interesting verification of the reliability of the peaks of the curves as indicative of preference, is seen in the modification of the curves in accordance with motives that are well known: the excess of the number of women over the number of men at the ages of 18 and 20, and the relative increase in the number of women in the early twenties and the relative decrease in the early thirties. Statistical expectation, of course, demands a relatively smooth curve, where the numbers are as great as are plotted in the curve, since the real ages of the population must be fairly regularly distributed, and but small and gradual changes in the relation of the two curves.

Lest the reader suspect that the enumeration of the Twelfth Census was not made with especial care, in order to decrease as much as possible these well known sources of error, we quote the Chief Statistician of the Thirteenth Census (1910):

> In 1900 both "date of birth" and "age at last birthday" were called for, one as a check upon the accuracy of the other, and this double return may have reduced somewhat the margin of error. . . . The possible gain in accuracy from this double return did not seem, however, to justify loading the population schedule—already complicated—with an additional inquiry. It was, therefore, determined to ask but one question . . . "Age at last birthday (before April 15, 1910)."[4]

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[2] Vol. 2, pp. xxxv ff.

[3] *Infra*, p. 232.

[4] Report of the Thirteenth Census, vol. 1, p. 291.

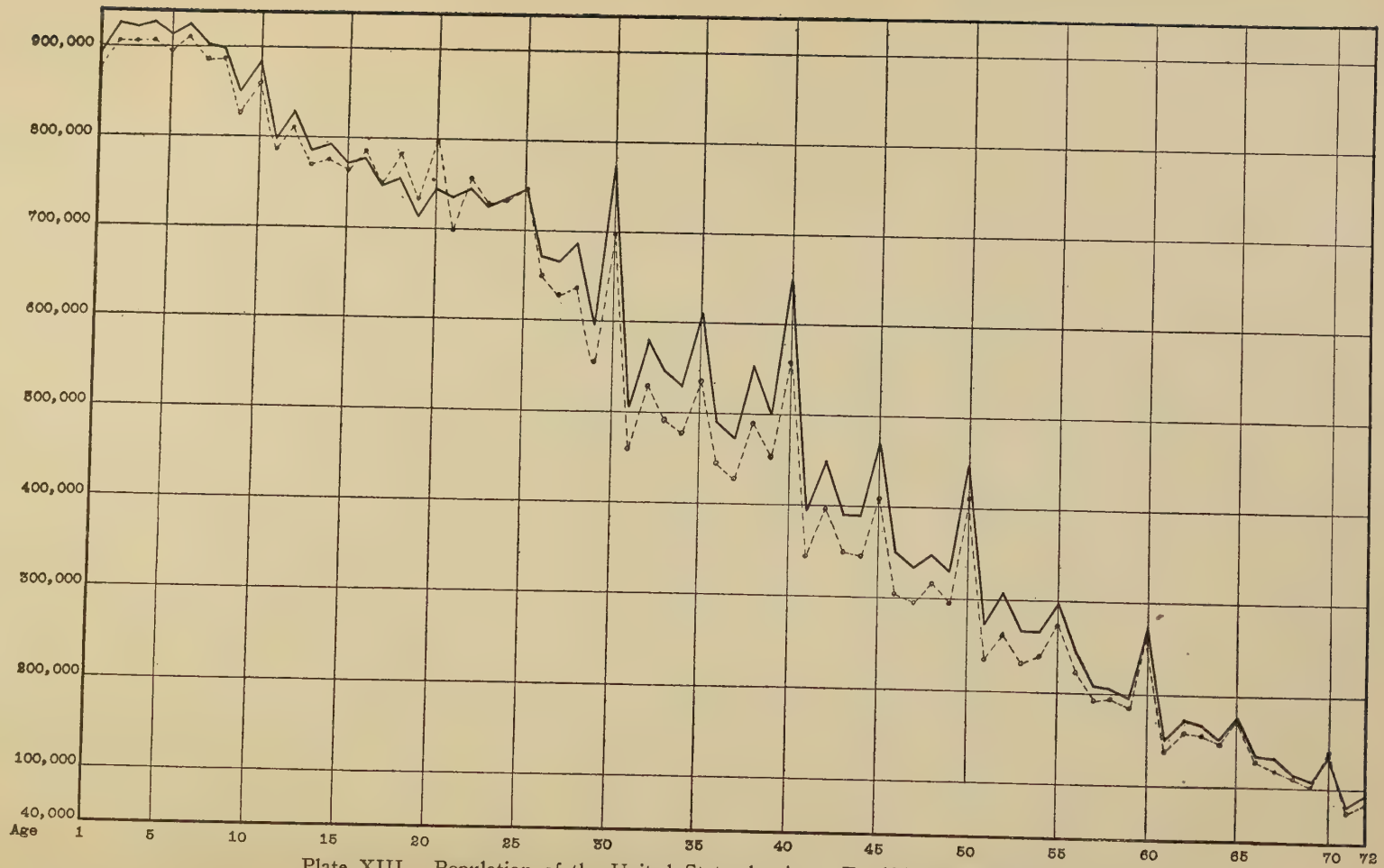

Plate XIII.— Population of the United States by Age. Twelfth Census (1900).
Ordinates give the number of persons; abscissæ give the age; solid line, male; broken line, female.
Enumerator required both age at last birthday and date of birth.

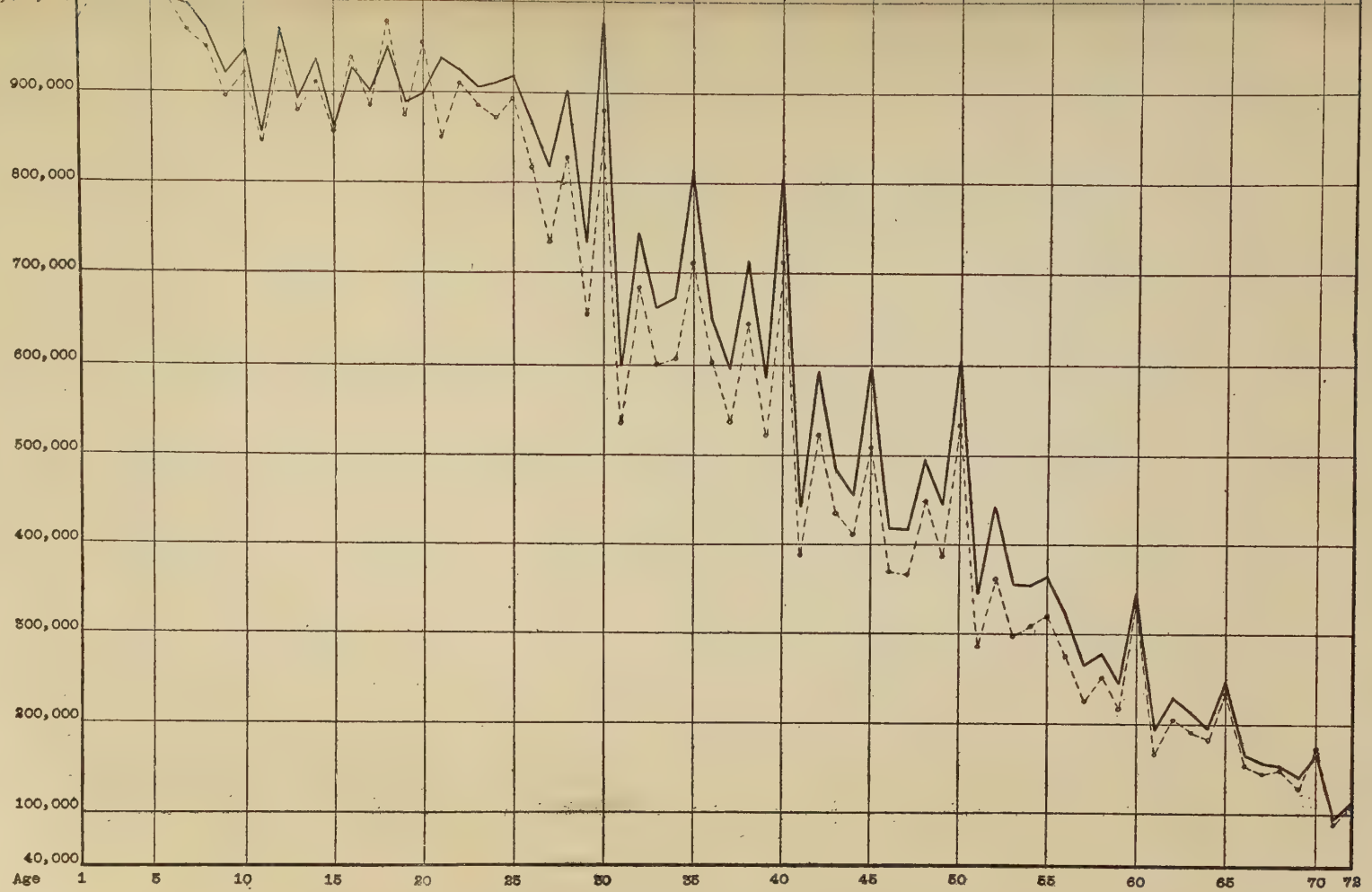

Plate XIV.—Same as preceding plate. Thirteenth Census (1910).
Enumerator required age at last birthday only.

And Plate XIV,[5] which gives the distribution curves for the Thirteenth Census, shows a considerable exaggeration of the number preferences already pointed out. For example, although there are at any time fewer men in the United States at the age of 30 than at the age of 29, the Twelfth Census lists 170,961 more, and the Thirteenth Census 238,010 more; and although there are likewise certainly fewer young women at any time at the age of 18 than at the age of 17, the Twelfth Census lists 34,723 more, and the Thirteenth Census 92,899 more.

We may conclude, therefore, from these two sets of distribution curves, (1) that there is a specific set of number preferences with respect to age-estimation, which is shared by people of all ranks in life; and (2) that those preferences are exaggerated when greater freedom is allowed in giving estimated rather than true ages.

Considering the fact that the bulk of the census returns were gathered from people in the lower and middle ranks of life, one might surmise that these number preferences rest upon mental inertia; that to an alert mind 29 is as likely an estimate as 30.

Now, I presume no class in the country possesses a higher intellectual rank, or minds more keenly alert, than the judges of our courts. If the judges show number preference in the terms for which they commit convicted prisoners to the penitentiary, the surmise must be wrong. It may fairly be expected that degrees in the gravity of crime will be distributed in a regular fashion, would be represented by a smooth curve over a time-scale, and that judges exercise care in graduating terms of commitment, within the limits assigned by the law, to the gravity of the crime.

Terms of Criminal Sentences.

From the Eleventh Census (1890), which provides the most complete available data concerning crime, we learn that there were 63,653 prisoners serving term sentences in our prisons. The terms of commitment, for an integral number of years, are shown in Plate XV. Curve *A* shows the distribution of the terms for an integral number of years from 5 to 25; Curve *B*, from 20-40; Curve *C* for fractional parts of a year, given in addition to one or more years; and Curve *D* for an integral number of months.

An examination of these curves shows that the terms of commitment of our prisoners were determined in a marked degree by the judges' preference for round numbers in years, and for quarters of years in months.

[5] *Supra*, p. 233.

It has been suggested, in palliation of the judges' position, (1) that the law provides for any given crime, lower and upper limits of possible sentence between which the round numbers furnish natural and sufficiently fine gradations; and (2) that the judges are fully aware of their frequent preference for the round numbers and do not feel under obligations to use finer gradations.

With respect to the first point, in order to determine whether judges often gave the limit, either upper or lower, and whether the limits assigned by the laws of the respective states for the various crimes were

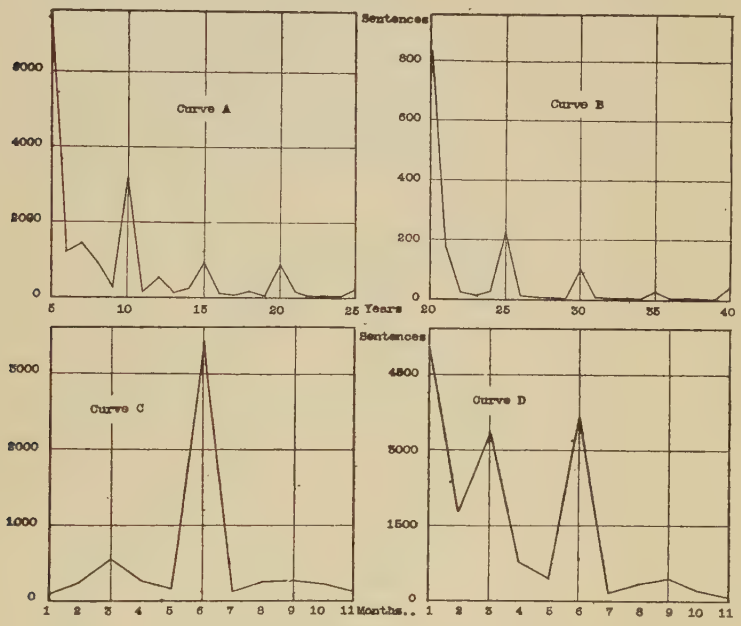

Plate XV.— Terms of Criminal Sentence in the United States. Eleventh Census (1890).

usually in round numbers, the census tables were again searched, with the following results:

(*a*) Both minimum and maximum sentences allowable ranged very irregularly from less than a year to life, and preferences were shown for 1, 2, 3, 5, 7, 10, 14, and 20 years, and life, the stronger preference above 4 years being the maximum sentence of 10 years.

At just one point does the preference in a limit depart markedly from the preference in term of commitment: 14 years is preferred over 15 years in the former, while 15 is preferred over 14 in the latter;

which would tend to impute an independent number-preference to the judges.

(*b*) Assuming that if judges often gave a limiting sentence it would probably be shown in granting the lower limit to women, a crucial case was sought. There were 257 female prisoners committed in 26 respective states for grand larceny. Plate XVI shows the curves of the minimum and maximum sentences allowable, in the respective states, and of

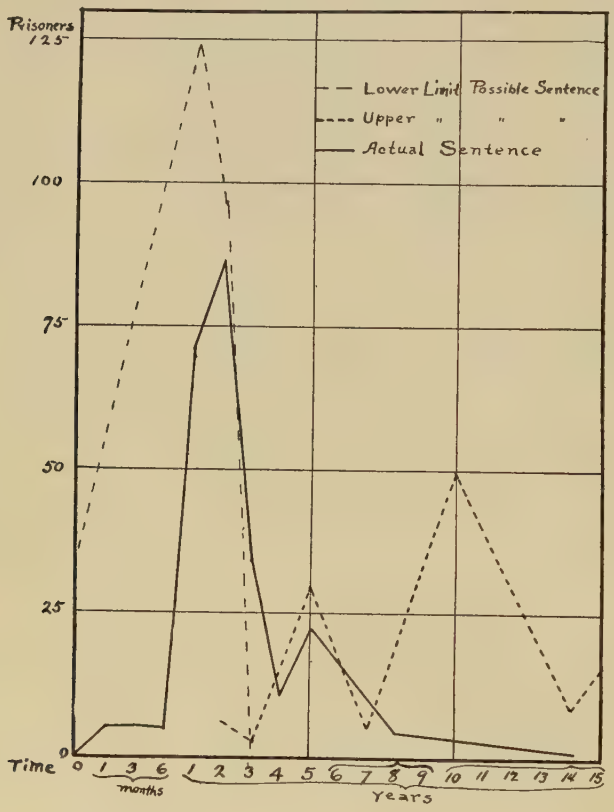

Plate XVI.— Female Sentences for Grand Larceny.
Eleventh Census, 257 cases.

the terms of commitment. The latter is shifted above the minimum sufficiently to show that the minimum sentence was seldom given. At one point, on 5 years, the commitment curve conforms to the maximum curve: but those prisoners were sentenced in states in which the maximum sentence allowable was 7, 10, 14, or 15 years. Again, the commit-

ment for 5 years was determined by an independent preference for a round number on the part of the judges.

(*c*) The tables were searched for single commitments for the respective crimes in the respective states, and 58 cases were found. Plate XVII shows pictorially the range of allowable sentence and the term of

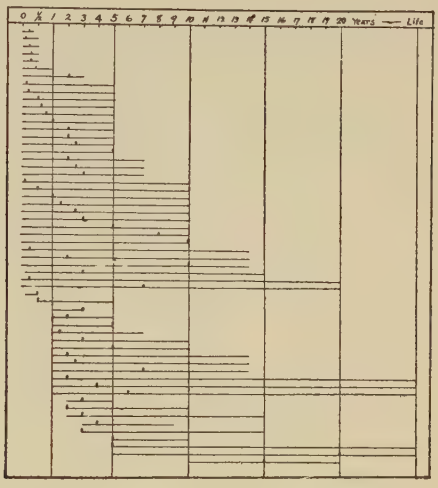

Plate XVII.— Single Commitments. Eleventh Census.
The lines show the range of sentence permissible by law;
the vertical stroke shows the sentence pronounced by the judge.

commitment, for each case. In five cases the minimum sentence was given; in five cases the maximum was given. Only about a sixth of the sentences were determined by a limit assigned by law.

It is more than probable, therefore, that the preference for round numbers shown in the distribution of terms of commitment has not been imposed upon the judges by the limits of sentence assigned by law.

As to the round numbers furnishing sufficiently fine gradations, between the imposed limits, for an equitable graduation of term of commitment to gravity of crime, it may be admitted, perhaps, that such might be the case for sentences above 25 or 30 years; but it can scarcely be admitted for sentences under 20 years. It would seem that it must make a sensible difference to a prisoner whether he is sentenced for 18 or for 20 years, and still more whether he is sentenced for 5 or for 7 years. That such distinctions are recognized by judges is indicated by the fact that they do make still finer ones, since, as the distribution of commitments shows, every integral number of years between the

round numbers has been pronounced in one or more cases up to 43, and in more than four cases up to 37; and not only so, but up to 22 years fractional parts of a year have been added to each of the integral numbers, in order to make the gradations finer, as the two samples in the following table illustrate:

Number of Sentences	Years	Months
2	19	4
2	19	5
6	19	6
1	19	10
2	19	11
1	9	2
8	9	3
4	9	4
3	9	5
32	9	6
5	9	8
6	9	9
9	9	10
3	9	11

Perhaps the evidence in the distribution of terms of commitment is itself sufficient to indicate that gradations in round numbers below 20 years are recognized by the judges to be too coarse to meet the ends of justice; but if so, the extraordinary preference for round numbers exhibited by judges must be in part if not largely an involuntary preference,—the subliminal influence of a mental habit upon judgment. This suggestion gathers force from the fact that others whose sense of justice has been less carefully cultivated than that of judges recognize in the situation something reprehensible to the bench.

Hewes, after discussing the distribution of criminal sentences, says:

> Of course no man, even including the judges themselves who pronounce sentences, would undertake to claim that criminality is graduated so irregularly as are the terms of sentence. If, then, the calmest, the best-educated judgments deviate so far, in suiting penalty to guilt, what wonder if ordinary judgment in less serious affairs of communities makes many mistakes.[6]

Concerning criminal sentences in England, which are given in weeks up to 11, in months from 3 to 35, and in years from 3 on, and which exhibit similar number preferences for 2, 5, and 9 weeks, 3, 6, 9, 12, 18, and 24 months, and 5, 7, and 10 years, Sir Francis Galton, after study-

[6] F. W. Hewes: The influence of number in criminal sentences. *Harper's Weekly*, 1896, 40: 254.

ing the irregular distribution of 54,670 sentences (43,300 in weeks, 10,540 in months, and 830 in years), said:

It would have been expected that the various terms of imprisonment awarded by judges should fall into a continuous series.

Such, however, is not the case. . . . It is impossible to believe that a judicial system acts fairly, which, when it allots only 20 sentences to 6 years imprisonment, allots as many as 240 to 5 years, as few as 60 to 4 years, and as many as 360 to 3 years. Or that, while there are 20 sentences to 19 months, there should be 300 to 18, none to 17, 30 to 16, and 150 to 15. . . . Runs of figures like these testify to some powerful cause of disturbance which interferes with the orderly distribution of punishment in conformity with penal deserts.

On examining the diagram [Plate XVIII] we are struck with the apparent

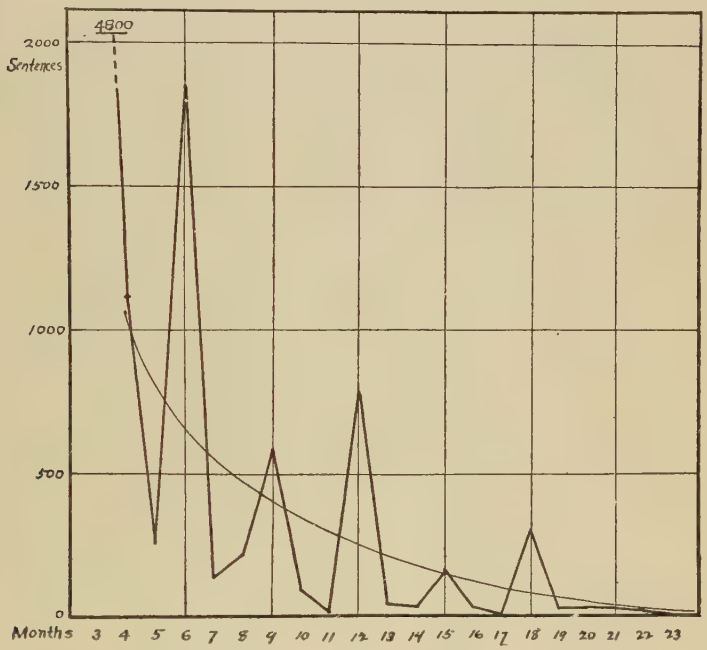

Plate XVIII.— Terms of Criminal Sentence in England.
(From data published by Galton.)

facility of drawing a smooth curve, that shall cut off as much of the hill-tops of the irregular curve as will fill their adjacent valleys. . . . The smoothed curve may therefore be accepted as an approximate rendering of the general drift of the intentions of the judges as a whole, and shows that the sentences passed by them severally ought to be made more appropriate to the penal deserts of the prisoners than they are at present.[7]

[7] Galton: Terms of imprisonment. *Nature*, June 20, 1895, pp. 174-6.

It is consequently unlikely that judges, although they doubtless vary considerably in this respect, are conscious of, and acquiesce in, the large control of their judgments by the round-number mental habit. But we shall not rest the case for "unconscious" influence of mental habit upon this evidence alone.

If there is any sphere in which such an influence works unconsciously, it must be in our educational institutions where conscientious teachers give their students final grades in their respective subjects, or in science where the observer or experimenter endeavors to the best of his ability to give an impersonal and accurate judgment in his report of the facts of nature.

Estimates of Star-Magnitudes.

The astronomer, for example, desires a true report of the magnitudes of the stars, and would not welcome the control of judgment by a mental habit. For centuries, of course, all of the visible stars (down to the sixth magnitude) have been known by name and their degrees of brightness estimated by various observers. But reports, owing to the many variable conditions of observation, such as moon-light, proximity of brighter stars, milky-way, declination of the star at time of observation, atmospheric absorption, etc., and the observer's characteristic mental scale of magnitudes, lacked uniformity to such a degree that in the latter half of the nineteenth century it was agreed among the astronomers of the world that a complete re-estimation of all stars, both visible and telescopic was necessary; and the tremendous undertaking was parceled out to the various great observatories. Suppose we turn to the preface of a report on the visible stars which was made by one of these observatories, in order to learn from Professor E. C. Pickering how the list of 4,260 stars, which is to demand our attention, was made by selection from seventeen existing catalogues or star-lists, and what aims directed the observations:

> An extensive and systematic inquiry, by photometric methods, into the comparative brightness of the stars, has long been regarded among astronomers as highly desirable. . . . The observations to be described in the present volume were planned with a view of obtaining a satisfactory determination of the comparative brightness of all stars not fainter than the sixth magnitude, which could conveniently be observed in this latitude. In the absence of a definite and generally accepted scale of magnitude, it was impracticable to include in the list of stars to be observed all those surpassing a certain degree of brightness, and to exclude all others. The course adopted, therefore, was to form the required list by the selection from several standard catalogues of stars estimated in any one of these catalogues to be of the first six magnitudes. In consequence of accidental errors

of estimation in one or another of the catalogues thus consulted, many of the stars included in the present work are no doubt fainter than others which have been excluded; but if any stars are omitted which should actually be regarded as of the sixth magnitude, they must have been under-estimated in all these catalogues.[8]

The Meridian Photometer is described[9] as an instrument with a horizontal eye-piece and systems of prisms and lenses such that the images of the pole-star and of the star to be estimated could be brought into the center of the field for direct comparison; and the method of observation, requiring two persons, provided for the estimation of inequality to be read from a scale with gradations in tenths of a degree. Thus the observer's error was reduced to a minimum: he brought the two images close together, turned a "Nicol" prism until the observed star was judged to be equal in brightness to the pole-star; the recorder took the reading from the scale over which the indicator of the "Nicol" turning mechanism played; the observer then turned the corresponding prism until the pole star just disappeared, then regained a brightness equal to that of the observed star; the recorder again took the reading from the scale; the images were reversed in position, and the procedure was repeated. From the average of the readings the brightness of the star was determined to a hundredth-magnitude. Any error on the observer's part would have to be limited to the mental process of judging the equality of two visible images, after the errors peculiar to "The Method of Minimal Changes in its Application to Stimulus Comparison,"[10] have been eliminated, and may be considered entirely negligible.

The magnitudes in this catalogue, consequently, approach scientific accuracy, and their distribution may be readily accepted as a true distribution of the real magnitudes of the visible stars.[11] They are listed, however, beside the magnitudes published in several other catalogues, chief among which is Argelander's Durchmusterung, published from the great observatory at Bonn during 1852-1861.

This catalogue includes all the stars down to the 10th magnitude, from the pole-star to 2° beyond the equator. The stars common to the two lists, therefore, would be all the visible stars (or stars listed in one

[8] Observations with the Meridian Photometer during the years 1879-1882. *Annals of the Astronomical Observatory of Harvard College,* Cambridge, 1884, 14: Preface.

[9] *Op. cit.,* pp. 1-9.

[10] *Vide,* Külpe: Outlines of Psychology. London, 1901, pp. 56-7.

[11] For the elaborate provisions made for eliminating the errors due to varying objective conditions of observation the reader is referred to the *Annals.*

or more of the 17 catalogues as the 6th magnitude or brighter) in the northern heavens and down to 2° beyond the equator. These stars, 2884 in number, furnished the basis of our calculations. They were tabulated, by magnitude, according to both estimates; the Photometric estimates being taken to the nearest tenth-magnitude (as 5.54 = 5.5, and 5.56 = 5.6; but 5.55 = 5.6, and 5.65 = 5.7*).

A comparison of the distributions of the Photometric and the Durchmusterung magnitudes (for the 2884 stars common to both lists), shown in Plate XIX,[12] is interesting and instructive. The latter curve shows the characteristics which we have already observed in the distributions of ages and of prison-sentences; a marked preference is shown for the round numbers.

This fact had already been noticed by Professor C. S. Peirce, who offered a very interesting explanation:

> The Durchmusterung is one of the best collections of magnitudes which we have; and here are some of the numbers of stars of different magnitudes according to that catalogue:
>
> | Magnitude | Number | Magnitude | Number |
> |---|---|---|---|
> | 6.0 | 618 | 6.6 | 159 |
> | 6.1 | 106 | 6.7 | 457 |
> | 6.2 | 293 | 6.8 | 901 |
> | 6.3 | 275 | 6.9 | 137 |
> | 6.4 | 101 | 7.0 | 2,141 |
> | 6.5 | 1,239 | | |
> | Total | | | 6,427 |
>
> It is impossible to believe that the stars are really distributed in the heavens in this singular way; so that there are really 1239 stars of the 6.5 magnitude and only 159 of the 6.6. My father has already introduced into another branch of astronomy the principle that the magnitudes of the different parts into which an observer divides a scale in estimating tenths are respectively proportional to the numbers of cases in which the figures denoting them are found to occur. Thus if we find in any observations of transits by eye and ear, twice as many transits the time of which ends with 0.5 sec. as of those which end with 0.4 sec., we assume that in that observer's mental subdivision of the second the fifth part was twice as long an interval as the fourth. We may extend the same idea to the comparison of scales of star-magnitudes. If one observer says there are 9 first magnitude stars in the northern heavens and another finds only 8, clearly the latter consigns some star to the 2d magnitude which the other considers to be of

* Which differs from the conversion from hundredths to tenths in the catalogue, where a magnitude with a 5 in the hundredth's place was changed to the nearest *even* tenth: 5.55 = 5.6; but 5.65 = 5.6. For this reason, were the converted magnitudes in the catalogue tabulated, an artificial excess upon the even tenth magnitudes would disturb the distribution.

[12] *Infra*, p. 243. The magnitudes are given in tenths.

the 1st, and therefore he makes the limit between the first and second magnitudes to be brighter than the other makes it. Suppose that neither of the two observers made any errors in his estimations and that their discrepancies arose solely from the differences of their scales of magnitudes. Then if they observed the same stars, whichever had fewer stars brighter than the 4th magnitude, for example, would have made the limit between his third and fourth magnitudes the brighter.[13]

Professor Peirce had his aid, Mr. Farquhar, re-tabulate all of the Durchmusterung estimates down to the 6.1 magnitude, and by adding K. von Littrow's tabulation down to the 9.5 magnitude he constructed a table from which he calculated an "equitable distribution" of the 314,925 stars in the list, such as one might expect to show the actual distribution of the stars in the heavens,[14] and calculated the theoretical magni-

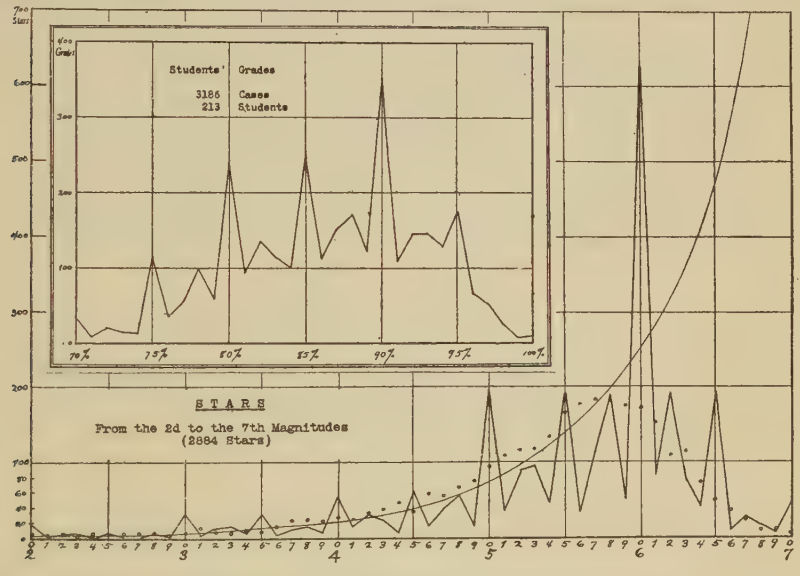

Plate XIX.— Magnitudes of the Stars reported by various observers to be as bright as, or brighter than, the 6th magnitude.

The solid line gives the estimates in Argelander's Durchmusterung; the smooth curve gives the theoretical distribution (*vide*, footnote 14); the small circles give the Harvard photometric determinations.

—Students' Grades, from applications for entrance into Stanford University. 3186 Cases.

[13] Photometric Researches, 1872-75. *Annals of the Astronomical Observatory of Harvard College*, 1878, 9: 8-9.

[14] The smooth curve in Plate XIX is a limiting curve estimated from this table, published in the *Annals* on p. 26. The true magnitudes should be found either in the curve or just above it, if the stars are "equitably distributed" in the heavens.

tudes, thus reducing the Durchmusterung magnitudes to the scale of equitable distribution. From the curves in Plate XIX one may see how much the Durchmusterung distribution departs from the theoretical, when the magnitudes down to the 6th are considered; and also how the theoretical approximated fairly closely the real distribution as later determined by Photometric observations.[15]

But were we to take at their face value the regular departures of the Durchmusterung curve from the Photometric, we should be doing injustice to the Bonn observatory and we should also lose an opportunity to consider some facts which will be of assistance in a final analysis and explanation of the round-number mental habit.

A letter[16] from Professor Schönfeld, at Bonn, written in explanation of the manner in which the Durchmusterung estimates were made, informs us that during one period of observation no especial effort was made to estimate magnitudes in smaller gradations than half-magnitudes; during another, only descriptive words were added to the half-magnitude estimates; and only finally did the observers endeavor to estimate to the tenth of a magnitude. All estimates were then converted into magnitudes expressed to a tenth. The first method contributed about 20% of the estimates; the second about 50%; and the last about 30%.

This would account for much of the deviation of the Durchmusterung curve, but not necessarily for the greater part of it. If we assume that all three methods, in their stated proportions, contributed to the distribution of the magnitudes from 6.0 to 7.0 quoted above from Professor Peirce, and deduct proportionally from the numbers of estimates of the magnitudes 6.0, 6.5, and 7.0, a number of cases (1820) equivalent to 20% of the total (which is 6427) plus a sixth of 50% of the total, we shall have made a rigorous correction for the number of

[15] It would seem that, after all, not all of the stars down to the 6th magnitude were included in the Harvard list, since its distribution breaks from the theoretical before the 6th magnitude is reached: Roughly one might estimate that at least 15 stars of the 5.8, 45 stars of the 5.9, and 100 stars of the 6.0 magnitudes were omitted. In this case, however, as was observed by Professor Pickering, those stars would necessarily have been omitted in each of 17 respective catalogues, from which the list was compiled. At any rate, since there must be more stars of the 6.0 magnitude than of the 5.9, or of the 5.8, the peak of the distribution curve should fall on the 6.0 abscissa, if all stars as bright as the 6th magnitude are included.

[16] Published in the *Annals*, 1878, 9:27.

estimations not intentionally made to the tenth of a magnitude. This gives the corrected distribution:

6.0	335	6.6	159
6.1	106	6.7	457
6.2	293	6.8	901
6.3	275	6.9	137
6.4	101	7.0	1,141
6.5	873		

Plate XX gives the curve of the distribution, in relation to the estimated limiting curve of "equitable distribution" for the original number

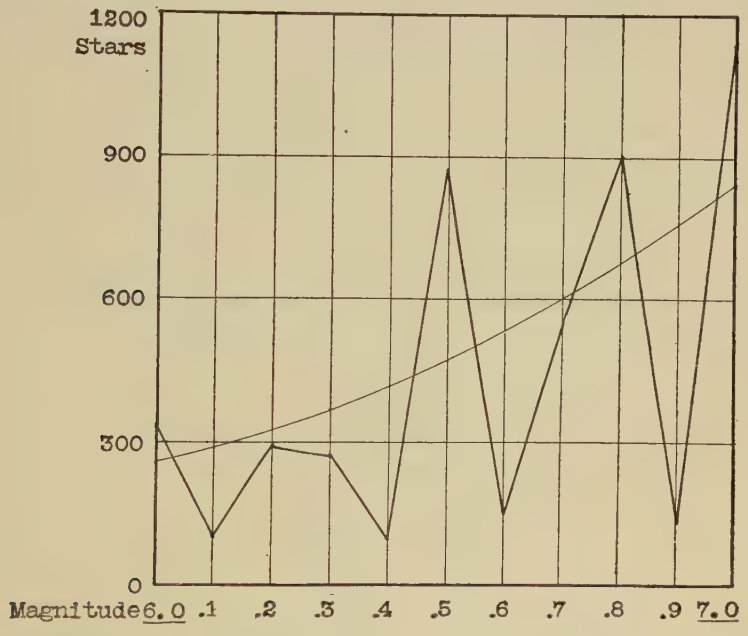

Plate XX.— Corrected Distribution of the Durchmusterung estimates of stars of 6.0 to 7.0 magnitudes.

of cases; the ordinate on 6.8 indicates that the correction has probably been too great, yet the round-number estimates are still in considerable excess over the others, tending to show that the influence upon the astronomer's judgment was unconscious, when he endeavored to assign the star-brightnesses to the tenth of a magnitude.

This evidence is supported by the direct testimony of Professor Schönfeld in his letter to which reference has already been made. He said that even from the time observations were begun to be made to the tenth of a magnitude until the end of the work "the decimals 1, 4, 6,

and 9, but especially 1 and 6, were much less often estimated than the others." [17]

One of the principal reasons that Professor Schönfeld, in the same letter, offered for the earlier methods of observation is enlightening, and will no doubt be found to underlie much of the evidence for the round-number habit. He said: "We could not discriminate between the magnitudes 7.4, 7.5, and 7.6, for example, with the same degree of certainty as between 7.3 and 7.4, or 7.6 and 7.7." There is an inter-class field about the division-marks (here, half-magnitudes) in the mental scale, within which discrimination is felt to be difficult; but differences no greater, between values in adjoining inter-classes, are felt to be readily appreciated. In accordance with this feeling one might expect an excess of estimates upon the mid-points of the inter-class fields, *i. e.*, upon the division marks. This is probably a normal situation when estimates are made in accordance with a mental scale. Whether the feeling is justified in objective fact, or is illusory, is another matter, which depends upon the stability of the mental scale and upon distinguishable differences.

The inferences drawn from our data in the above paragraphs have been verified in data published by E. Grossmann, of Kiel.[17a] He calcu-

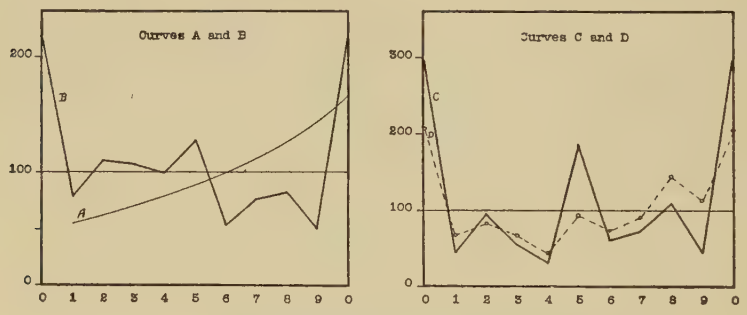

Plate XX*a*.— Estimates of Star-Magnitudes.
Curve *A*. Seeliger typical distribution. (From Grossmann.)
B. Observer J. " "
C. Early observations by H. " "
D. Later observations by H. " "

In order to show the relation between 0 and 9 as well as between 0 and 1, the 0 is repeated after the 9. The most probable distribution is shown in the lightly-drawn ordinate.

[17] "Doch ist dabei zu bemerken, dass auch dann, und bis zum Schluss der Arbeit, die Zehntel 1, 4, 6, und 9, besonders aber 1 und 6, viel seltener geschätzt wurden, als die übrigen." *Loc. cit.*

[17a] Grossmann: Ueber Schätzungen nach Augenmass. *Astronomische Nachrichten*, Kiel, 1906, 170: 159-160.

lated from the Seeliger typical distribution of the fixed stars the number of stars there should be for each tenth magnitude down to *9.0*. He then distributed these hypothetical stars of all magnitudes over the decimal scale (.0-.9), getting the values plotted in Curve *A*, Plate XX*a*. The deviations of the observations of two observers (*J.* and *H.*) from this typical distribution were found. If the typical distribution is represented by the light horizontal ordinate (at 100 per 1000), the deviations resulting from mental habit in the observations of *J.* are shown in Curve *B*; and those of *H.* in *C* and *D*. The "U" form of the curves indicates a strong preference for the *0* tenth; and in Curves *B* and *C* the *5* tenth is preferred. The similarity of Curves *C* and *D* indicates the permanent nature of an observer's mental habit; the observations in the two sets were made five years apart.

Students' Grades.

Another class of judgments, made upon the basis of a mental scale, which might be expected to be fairly free from the conscious influence of mental habit, is the final grades which teachers assign in the form of per cent to students in their respective subjects, especially when these grades are designed to reveal accurately the scholarship of the student and are expected to be used in his application for entrance into the university. Through the courtesy of the Registrar's Office [18] we were enabled to tabulate 3186 percentile grades from the applications of 213 students. The applications came from all over the country, and the grading was done by perhaps not less than 1000 different teachers. The distribution curve is shown in Plate XIX,[19] adjacent to the distributions of the star-magnitudes, with which it compares most favorably as evidence for the round-number habit.

It may be argued, however, that with teachers as with the judges, many of them consider the five-unit gradations sufficiently fine. But like the judges they do use the finer gradations, as the distribution shows, and as a class they are too conscientious to neglect a means of refining justice beyond a point which would challenge criticism from both pupils and parents. Indeed, fractional per cents are not uncommon in practice. I doubt if any teacher accustomed to grading by the percentile scale, who has not made a statistical study of the process, will be ready to claim that the five-per-cent divisions are sufficiently close

[18] For which acknowledgment is due particularly to Assistant Registrar John Ezra McDowell.

[19] *Supra*, p. 243.

for grading.[20] And yet no one will defend our distribution curve, as against a smooth curve, as representative of the real distribution of merit. The evidence seems fairly good for unconscious influence of mental habit upon judgment.

Of course, if the scale were linear and visible, like a meter-stick, and if the observed merit of the student were definite, like the edge of a chalk-box, estimates to the nearest centimeter would distribute smoothly, free from round-number preferences. It is only because the degrees of observed merit are sufficiently indefinite to migrate upon the mental scale that greater frequency upon round numbers is possible. Indeed, there is a fair probability that the least discriminable gradations upon the mental percentile scale lie about 8% apart.[21]

But if the length of a chalk-box were to be estimated by a single observer a great number of times, or by a large number of observers, to tenths of a millimeter, a distribution of the estimates might reveal the influence of the round-number habit. Here the scale is objective and the estimate is limited to the range between calibrations. Then, if the

[20] Only about ten applications were discarded on account of uniformly round-number grading.

[21] To determine the minimal distinguishable steps on the mental percentile scale, Professor Starch had "two papers in English work graded by 142 teachers of English and one paper in geometry graded by 118 teachers of geometry. The grades of one English paper ranged from 64 to 98 with a probable error of 4.0. The grades of the other ranged from 50 to 98 with a probable error of 4.8. The grades of the geometry paper ranged from 28 to 92 with a probable error of 7.5. To discover whether this wide variation might be due to the difference in standard among the schools, ten freshman English papers were graded by ten instructors of freshman English in the same institution. The mean variation of all these grades was 5.3. In order to eliminate the variation due to differences in standard among individual instructors, all the marks were weighted by the amount that each individual differed from the general average. The mean variation of these weighted grades was 4.2.

"In order to compare the accuracy of measurement by means of a mental scale in an entirely different field, five rods ranging from 10 to 23 inches were judged in terms of inches by eleven experienced carpenters. The mean variation . . . is identical with the variation of the grades, which indicates that the deviation of the marks is not due to the nature of the examinaton paper but it implies that measurements by means of a mental scale simply cannot be made any more accurately.

"The steps on a scale should be at least twice the size of the . . . probable error of the measurements in order to be distinguishable steps,"—approximately 8 points. Between the grades 60 and 100, the minimal number of distinguishable steps is five. (Reliability and Distribution of Grades. *Psychological Bulletin*, 1913, 10:74).

observers were men of science whose observations were expected to be permanently recorded, and used in the advancement of science, any regular deviations from a smooth curve would be regarded as decisive evidence for the unconscious influence of mental habit upon judgment.

Temperature on Pikes Peak.

Over forty years ago meteorological observations on Pikes Peak (the highest regular station then in the world), were made under the direction of the Chief Signal Service Officer of the United States Army, General A. W. Greely. The records for hourly observations of barometric pressure and of temperature, for the months of August and September, 1874, are available [22] for our inspection. Both sets of observations were tabulated according to the final digit (hundredths of an inch in the barometric readings; tenths of a degree in the readings of a Fahrenheit thermometer). The barometric records were found to be subject to great and apparently regular variation in the distribution over the digits from 1 to 0; but since they had evidently been corrected for both temperature and altitude, they are unserviceable for determining what the preferred digits were. The distribution of 975 thermometer-readings is shown in curve *A*, in Plate XXI.[23] The "U" form of the curve indicates the preference for the *0* deviation from the calibration-mark on the scale, the increasing insecurity of the estimate as the objective temperature departed from a calibration, and in this field of insecurity a preference for *5*.

Temperature in Mauritius, in the Greenwich Observatory, in Hertfordshire, and in Dundee.

A marked preference for the even and the half degree in thermometric readings returned from newly established second-order meteorological stations in Mauritius (a British island in the Indian Ocean) suggested to Albert Walter (of the Royal Alfred Observatory) the advisability of examining the distribution curve of the "tenths of estimation" for the purpose of testing the reliability of the observations.[24] From two stations 896 observations were received. Their distribution

[22] *Annals of the Astronomical Observatory of Harvard College*, 1889, *22*: 415 ff.

[23] *Infra*, p. 251.

[24] Albert Walter: On errors of estimation in thermometric observations. *Quarterly Journal of the Royal Meteorological Society*, 1909, 35: 249-257.

over the tenth degrees is shown in Plate XXI, Curve *B*. But to estimate their unreliability he needed a standard from experienced and competent observers. This he obtained from 4673 observations made in the Greenwich Observatory, during the years 1903-04, from the thermometers exposed in the Stevenson screen. The distribution of these expert observations is shown in the same Plate, Curve *C*. The deviations (from 100 per 1000) in the frequency of the various tenths in this curve started the author on a bit of calculation. Applying Pearson's formula of Closeness of Fit,[25] he found $\Delta = 14.37$, $P = \log 62.45705 - 100$; which indicated that the probability that the deviations were caused by chance is so slight that in its expression in decimal form the first significant digit is separated from the decimal point by 37 zeros.

John Hopkinson, who heard the paper incorporating these results read, at one of the Society's meetings, went home to Hertfordshire and subjected his own observations to a statistical inquiry which yielded some interesting results.[26] His distribution of 2000 observations (for 500 days) made near the commencement of his observations is shown in Curve *E*; and of an equal number of observations (for a like period) made about 20 years later is shown in Curve *F*. Then to determine whether the deviations were caused by reading the dry- and wet-bulb thermometers (vertical), or the maximum and minimum thermometers (horizontal), the observations were segregated and separately distributed as shown in Curves *G* and *H* respectively.

G. Hellmann [27] has recently published the distribution of the estimations in tenths in 7304 readings of the thermometer calibrated in single degrees, made at Dundee during the years 1901-10, which is shown in Curve *D*.

It is interesting to learn that although unskilled observers make greater systematic errors in thermometric readings than skilled observers do, the most expert observers make them, and make them in sufficient size to introduce serious error in the calculation of humidity from the readings of the dry- and wet-bulb thermometers. It is also interesting to note that the characteristics in the mental habit of an individual observer may stick to him during 20 years of experience, and apply to both vertical and horizontal scales. (The decrease of error on the hori-

[25] *Vide, supra,* p. 110.

[26] *Op. cit.,* p. 275.

[27] Hellmann: Psychologisch bedingte Fehler bei meteorologischen Beobachtungen. *Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften,* 1913, Erster Halbband, p. 286.

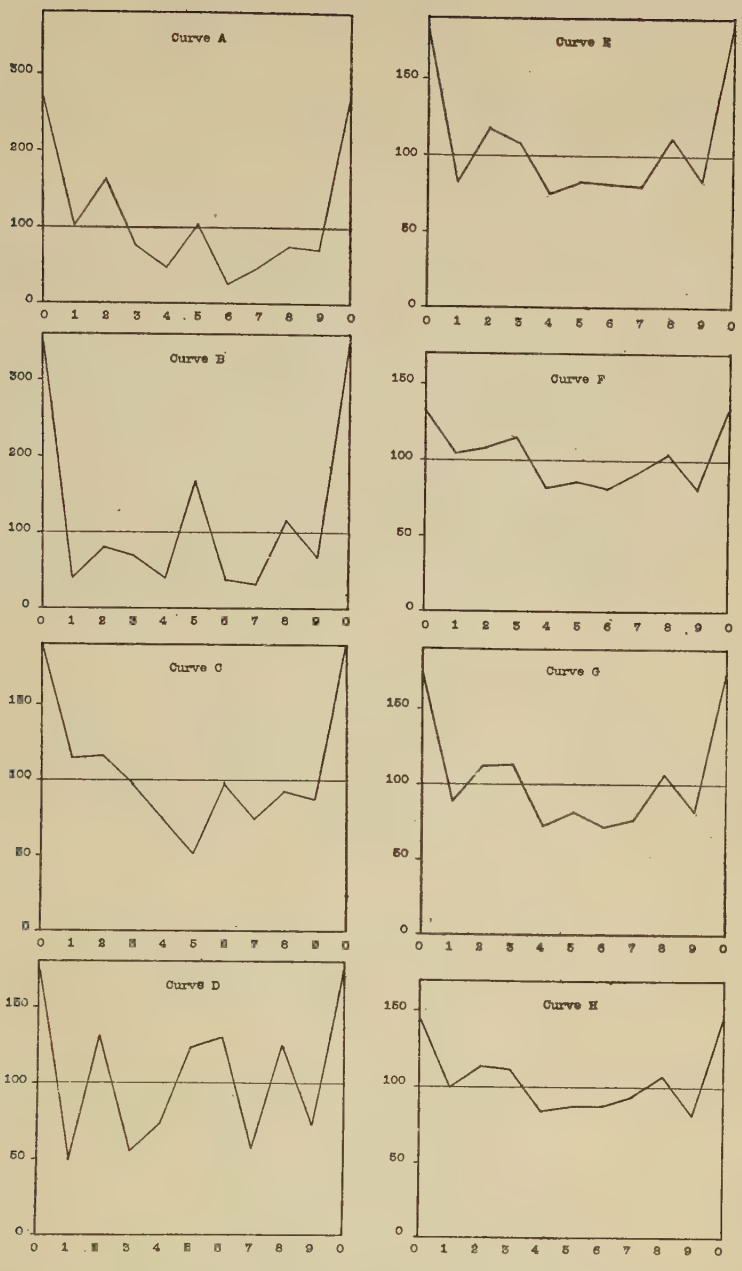

Plate XXI.— Thermometric Observations. Frequency distributions of the tenth degrees.

Curve *A*. On Pikes Peak. 975 cases. (Greely.)
B. In Mauritius. 896 cases. (From Walter.)
C. In the Greenwich Observatory. 4673 cases. (From Walter.)
D. In Dundee. 7304 cases. (From Hellmann.)
E. In Hertfordshire, earlier observations. 2000 cases. (From Hopkinson.)
F. In Hertfordshire, later observations. 2000 cases. (From Hopkinson.)
G. In Hertfordshire, vertical scale. 2000 cases. (From Hopkinson.)
H. In Hertfordshire, horizontal scale. 2000 cases. (From Hopkinson.)

zontal scale may be due to wider calibrations, or to the psychological advantage of a horizontal scale.)

Cloudiness at Bremen.

Albert v. Obermayer [28] has taken occasion to point out grave errors in the meteorological observations of cloudiness. He observes that, when cloudiness is estimated on a scale from *0* to *10*, it is unlikely that a wide deviation of the frequency on any given 10th, from the average, in one station's reports for a year, is a correct report, when the same deviation is not reported from neighboring stations; or that such a deviation in one station's reports for a period of years is correct. He gives the distribution of 2190 observations from Bremen for the years 1900 and 1901; 290 observations were *0*, and 754 were *10*. Excluding the latter because of its disproportionate frequency, we have remaining 1436 observations which distribute over the respective tenths as is shown in Plate XXII, Curve *A*. It forms a remarkably smooth "U" curve.

Rain-Fall in New England.

Curve *B*, in the same Plate shows like characteristics in the distribution of 1928 reports of Daily Precipitation from 53 stations, chiefly in New England, during the half-year January-June, 1888.[29] The estimates were made in hundredths of an inch, and only the records above (but not including) .10 were tabulated.

Estimates of Time from Kymograph Time-Records.

Curves *C* and *D* show the distributions of the estimates of time-intervals to tenths of a vibration in a kymograph time-record,[30] made by a statistical assistant and the writer, respectively. In the former distribution there are 5034 cases; in the latter, 1412. The observers were both chagrined upon learning the extent to which their judgments had

[28] Obermayer: Die Haufigkeitszahlen der Berwölkung. *Sitzungsberichte der Mathematisch-Naturwissenschaftlichen Klasse der Kaiserlichen Akademie der Wissenschaften*, 1908, Bd. 117, Abteilung IIa, pp. 217-229.

[29] Observations of the New England Meteorological Society in the year 1888, *Annals of the Astronomical Observatory of Harvard College*, 1889, 21: 76 ff.

[30] The electric time-marker was actuated at the rate of 31.75 times a second, and the individual vibrations were separated on the kymograph paper by a distance ranging from 1.25 to 1.67 mm. Estimates were made to tenths of this magnitude.

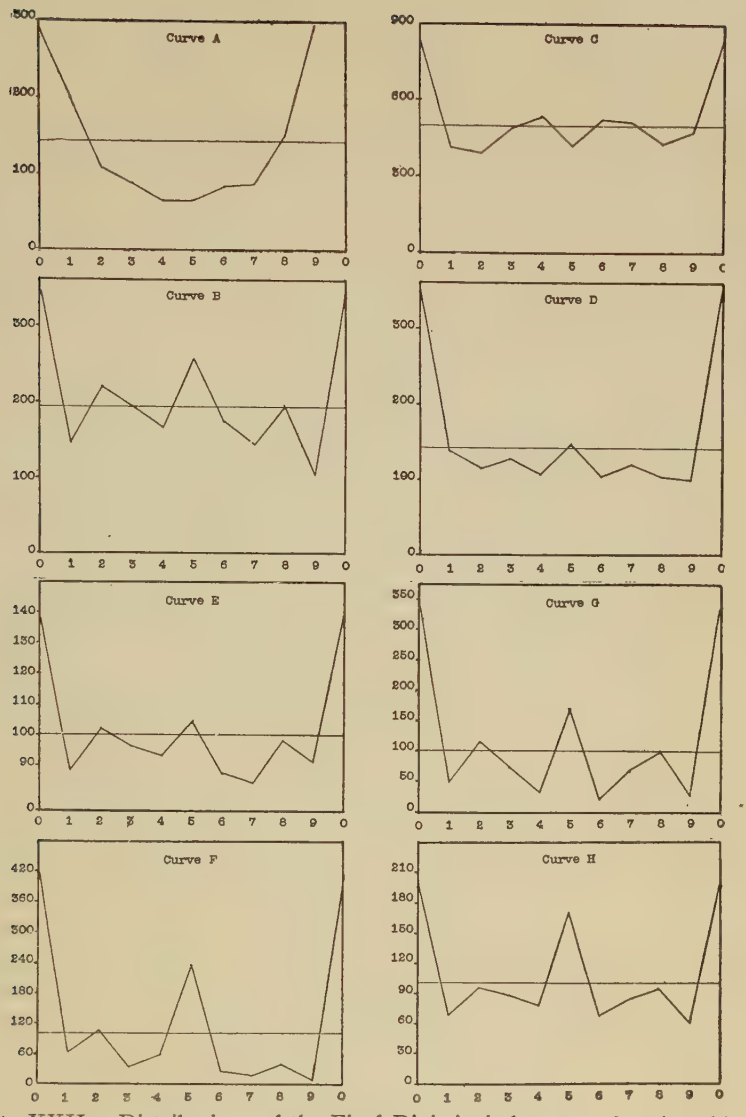

Plate XXII.— Distributions of the Final Digit in judgments of various kinds over the number series, to illustrate number-preference or the "personal scale."

The ordinates in curves *A-D* give absolute values; in curves *E-H*, relative values (per M). The lightly-drawn ordinate represents the most probable distribution or the mean.

Curve *A*. Cloudiness at Bremen. 1436 cases. (From Obermayer.)
B. Rain-fall in New England. 1928 cases. (New Eng. Meteor. S.)
C. Kymograph time-records. Obs. Dp. 5034 cases. (Stanford.)
D. Kymograph time-records. Obs. Cr. 1412 cases. (Stanford.)
E. Ages of men from 30-39 years. 5,498,871 cases.* (12th U. S. Census.)
F. Criminal sentences from 10-40 years.† 5018 cases. (11th U. S. Census.)
G. Star-magnitudes 6.0 to 6.9 from Plate XIX. 2619 cases.
H. Students' grades 70-99 from Plate XIX. 3154 cases.

* This curve is tilted back to horizontal by a reduction equivalent to the death rate; the number of cases is consequently also reduced.

† The plotted *o* value is ½ instead of ¾ the aggregate of sentences for 10, 20, 30, and 40 years,—a reduction which seems warrantable.

been influenced by the round-number habit. Plate XXIII shows the distributions of these regular deviations over the various magnitudes of time-intervals from 2.0 to 6.3. From curve *A* it may be noted that the

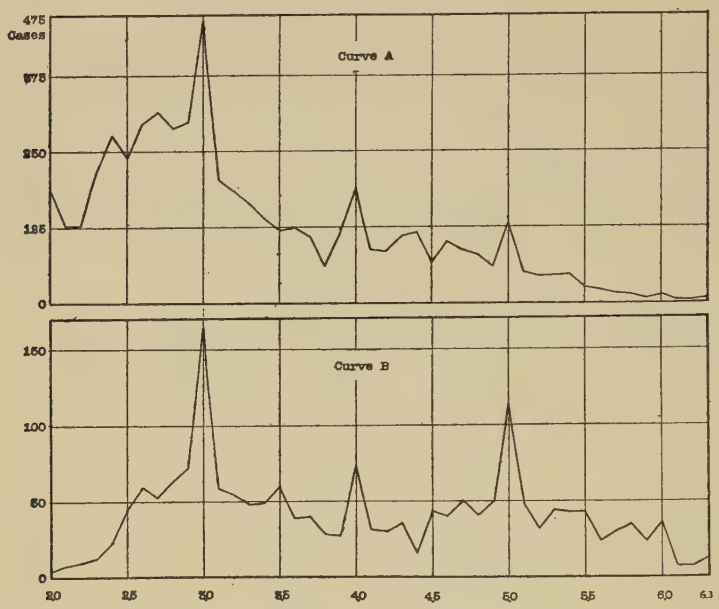

Plate XXIII.— Kymograph Time-Records. Curves of the frequency of estimates 2.0 to 6.3 units.
Curve *A*. Observer Dp. (Stanford.)
B. " Cr. (")

assistant has a negative deviation on the 5's, showing an individual characteristic, of which he was equally unconscious.

Curves *E*, *F*, *G*, and *H*, Plate XXII, show in the same form the corresponding characteristics in the census age returns, the criminal sentences, the estimation of star-magnitudes, and the percentile grades of students.

It is seen that whether the estimates are upon such values as ages, terms of sentence, students' grades, star-magnitudes, and cloudiness, or upon decimal values of a thermometer-scale, and spatial divisions on a kymograph-record, the frequency distributions present a common characteristic—shown by a "U"-form curve. In the scientific field this systematic error in estimates is unwelcome, which is sufficient evidence that the influence of mental habit upon judgment is here unconscious, subliminal.

How early this error in the estimation of the former class of values was noticed the writer does not know. It has probably been long known by census statisticians. But a signal case was pointed out in 1886 by J. Beloch,[31] who tabulated the ages of deceased persons from the Corpus Inscriptionum Latinarum. The distribution of 1809 cases over the unit's digit is shown in Table XXIV, Curve *D*. With respect to the latter class of values it was without doubt first noticed in the field of astronomy when the eye-and-ear method was employed for observing the time of the transit of a star. In 1858 Dr. Julius Hartmann[32] pointed out individual variations in the estimation of tenths of a second, which he attributed to individual habits in the manner of making the estimate, and observed that practice only serves to fix the individual habit responsible for the error more firmly upon the observer. He constructed an elaborate piece of apparatus designed to bring these habits to light and to provide practice for their correction. Professor Benjamin Peirce,[33] on April 12, 1859, read a paper before the American Academy in which he said that observations are not evenly distributed over the tenths of a second, and that consequently the mental time-scale does not correspond to the objective time-scale:

> The time-scale of each observer was inferred from the relative number of times which each tenth of a second occurred in his observations. It was shown that the habits of the observer were invariable in this respect, and were not subject to change with time or circumstance.

Curiously enough these specific references to an important error seem to have passed without notice until the matter was again brought to attention about thirty years later by several astronomers whose findings are worth our notice.

Estimations of Star-Transits.

By the eye-and-ear method of observation the observer watches the moving image of the star as it takes its course across the field in his telescope, locates its two positions occupied synchronously with the two successive strokes of the clock between which it traverses the spider's

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[31] Beloch: Die Bevölkerung der Greichisch-Römanischen Welt. Leipzig, 1886, S. 47.

[32] Hartmann: Einige Beobachtungen und Bemerkungen über Personaldifferenz. Grundert's *Archiv der Mathematik und Physik*, Greifswald, 1858, 31: 1-26.

[33] Peirce: Abstract of a memoir on the personal peculiarities of astronomical observers. *Proceedings American Academy of Arts and Sciences*, 1860, 4: 192.

web (the meridian), and estimates the ratio of the distance between the web and the first position in terms of tenths of the distance between the two positions. Since the latter distance represents a second of time, the estimate gives in a fraction of a second beyond the time of the clock at the first of the strokes the time, to a tenth of a second, of the transit. It is true that instruments vary; but the essential of the method is that the decimal part of a second is determined by an estimate of a decimal part of an imaginary line the ends of which are fixed by the positions of the image of the star when the clock strikes the two seconds nearest the transit-time.

If the estimates do not contain the systematic error which we have been portraying, they should, in an aggregate of a thousand or more, distribute fairly evenly over the respective tenths from *0* to *9*.

F. Boquet [34] tabulated a large number of observations made by three observers at the Paris Observatory during all seasons of the year and with three different instruments. The aggregated results for the three respective observers (*A*, 4000 observations; *B*, 3000; and *C*, 2000) are shown in Plate XXIV, Curves *A*, *B*, and *C*. He concluded that there is an error of estimation for each tenth which remains sensibly constant for each observer; and that this error varies from one observer to another.[35] This error he called *"l'équation décimale,"* which is the same as Peirce's "time-scale," or what has been more generally called the "personal scale," since it may refer to either space or time.

P. Brück [36] produced new examples, in considerable quantity, from his own observations: 2000 observations of stars in the equatorial latitude, made during 1889-90, and 3980 of stars in −26° and +68° made in 1890. The distribution of the aggregated observations is shown in Curve *E* of the same plate. He concluded that change in declination does not change his systematic errors.

G. Lewitzky,[37] of Charkow, published the distribution of 1868 transits of stars of the first four magnitudes, made by himself by the Zinger method, which is shown in Curve *F*. He supplemented Boquet's conclusions with the observation that always one of the intermediate tenths (0.4-0.7) is least often estimated by each observer.

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[34] Boquet: Recherche sur la valeur des observations de passages. *Bulletin Astronomique*, Paris, 1889, 6:337-343.

[35] *Op. cit.*, p. 341.

[36] Brück: Documents relatifs à l'équation décimale. *Idem*, 1890, 7:413-8.

[37] Lewitzky: Ueber den persönlichen Fehler bei Durchgangsbeobachtungen. *Astronomische Nachrichten*, 1890, 124:105-8.

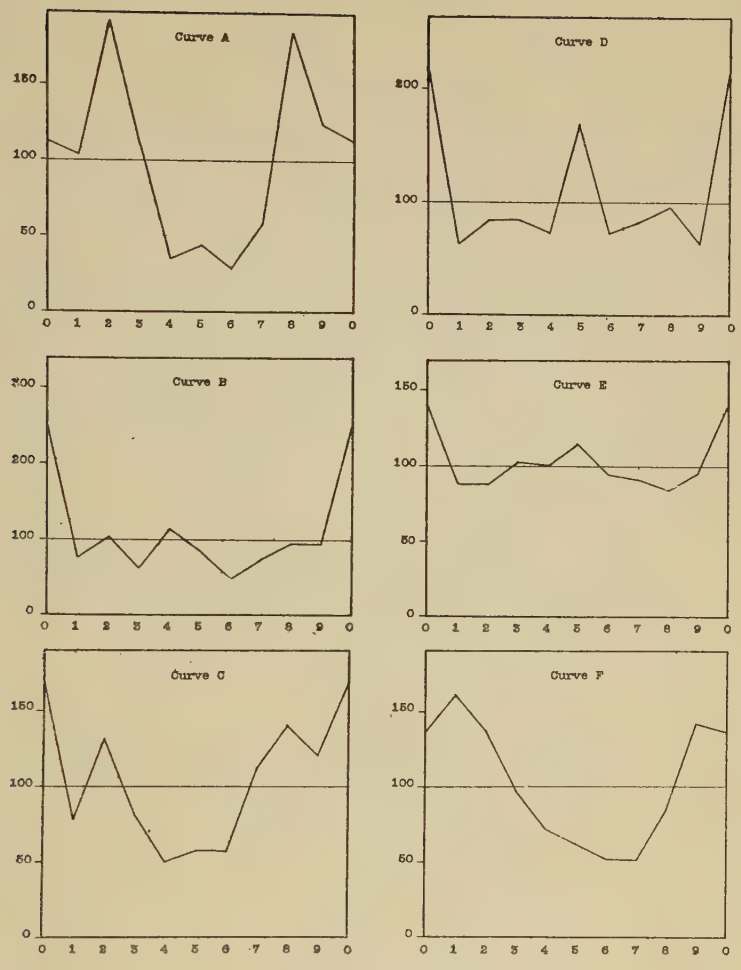

Plate XXIV.— Star-Transits. Distributions of tenths of a second, showing the *"équation décimale."*

Curve *A*. Paris Observatory, observer A. 4000 cases. (From Boquet.)
B. " " " B. 3000 " (" ")
C. " " " C. 2000 " (" ")
E. Besançon " Brück. 5980 " (" Brück.)
F. Charkow " Lewitzky. 1868 " (" Lewitzky.)
—Ages of the Latin dead.
D. Ages from the Corpus Inscriptionum Latinarum. (" Beloch.)

Otto Meissner,[38] of Potsdam, reported several distributions of a large number of observations of transits made by observers *N.* and *K.* The former of 8,505, and the latter of 16,215 observations are shown in Plate XXV, Curves *A* and *B.* Two sets, of 2249 and 2100 observations respectively, made by Observer *N.* in periods four years apart, show the permanence of this observer's *équation décimale;* their distributions are shown in Curves *C* and *D.* The distribution of a large number of observations (12,285) made by a third observer (*W.*) is of special interest since the distribution of the first set of 2069 observations exhibited the characteristics of the usual "personal scale" while that of the aggregate departs from them widely. Curve *H* (Plate XXV) shows the distribution of the aggregate in heavy line; Curve *J,* the distribution of the first set in light broken line; and Curve *K,* the distribution of the sixth set (1839 observations) in light solid line. The explanation is that the observer became aware of his systematic error, especially upon the *0* tenth and sought to correct it. The lesson is that the observer's consciousness of how his observations are distributed is not reliable. The neglect of *0* in the last set is greater than the preference for it in the first. The preferences of *2* over *1*, *7* over *6*, and *8* over *9*, are common to the three curves—persistent elements of the observer's "personal scale."

Boquet has shown [39] by the tabulation of a relatively small number of observations that the *équation décimale* remains fairly constant in the individual observer's use of different instruments. Observer *B.* made 712 observations with the *Cercle méridien du Jardin,* 946 with the *Lunette de Gambey,* and 1000 with the *Grand instrument méridien,* the respective distributions of which are shown in Plate XXV, Curves *E, F,* and *G.*

He also has shown [40] that the error of the observer persists whether he makes his observation by the eye-and-ear method, by the eye only, or by the ear only. One observer made 1000 observations each with the *Grand instrument méridien* (1) by the eye-and-ear method, (2) by reading the *Cercle*—eye only, (3) and by reading the *niveau*—eye only; and (4) he made 1000 estimates, in tenths of a second, by comparing the strokes of the second and the half-second siderial chronometers—ear

[38] Meissner: Ueber systematische Fehler bei Zeit- und Raumgrössenschätzungen. *Astronomische Nachrichten,* Kiel, 1906, 172:137-143.

[39] Boquet: Les recherches des astronomes sur l'équation décimale. *L'année Psychologique,* 1913, 19:27-65.

[40] Boquet: Sur l'équation décimale. *Bulletin Astronomique,* 1903, 20:165-170.

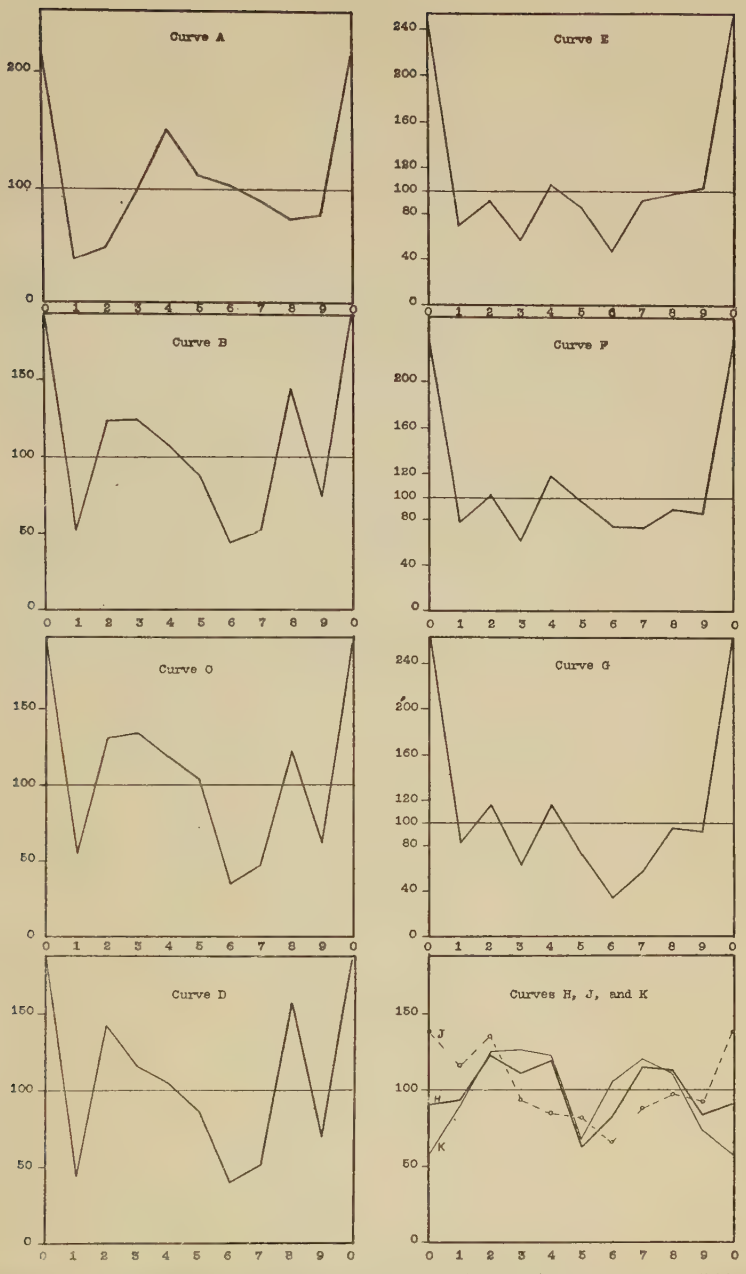

Plate XXV.— Star-Transits (continued). Showing independence of *"l'équation décimale"* of practice, instrument, and voluntary correction.

Curve								
A.	Potsdam	Observatory,	observer	K.	8505	cases,	(From	Meissner.)
B.	"	"	"	N.	16,215	"	"	"
C.	"	"	"	N.	2249 early	"	"	"
D.	"	"	"	N.	2100 later	"	"	"
E.	Paris	"	"	B.	Cercle; 712	"	"	Boquet.
F.	"	"	"	B.	Lunette; 946	"	"	"
G.	"	"	"	B.	Grand Instr. 1000	"	"	"
H.	Potsdam	"	"	W.	12,285	"	"	Meissner.
J.	"	"	"	W.	2069 early	"	"	"
K.	"	"	"	W.	1839 later	"	"	"

only. The respective distributions are shown in Plate XXVI, Curves *A, B, C,* and *D* (solid line). Curve *E,* superposed over Curve *D,* shows the distribution found by J. Plassmann [41] in estimating by ear tenths of seconds; although the two observers have characteristic differences their curves agree in type: the double "U" form—the objective unit was a half-second interval. Curves of this type represent the normal "personal scale" when the observations are made from instruments which have the *0.5* calibration, as may be seen from the adjacent Curves (*J* and *K*) which are drawn from data presented by G. Hellmann.[42] The solid line gives the distribution of 3652 observations of a 0.5° thermometer at Erfurt during the years 1901-10; the broken line, of an equal number of like observations made during the same period at Wasserleben.

Professor S. D. Townley, however, had previously published [43] distributions of his own transit observations, which indicated that the *équation décimale* persists, in part at least, after the observer increases his vigilance, and even after he turns from time-estimation by ear to space-estimation by eye. He explained that although he used the eye-and-ear method, with a three-inch Fauth transit and a chronometer beating half-seconds, he estimated "the *time* of the transit of the star, not the space passed over, to the nearest tenth of a second." His first series of 188 observations (see Plate XXVI, Curve *F*) revealed a strong preference for *0* and *5*, and a marked neglect for *4* and *8*. "Feeling sure that such large discrepancies should not occur," he determined to watch himself in future, and he carried out a second series of 816 observations (see Curve *G*) in which, although the deviations as a whole were much decreased, the preference for *0* and the neglect of *4* and *8* remained. And these three characteristics persist in a third series of 211 observations with the chronograph, by which the estimations were made from the chronograph sheets by eye (see Curve *H*).

It may perhaps be properly remarked here that the mental habit of the *équation décimale,* or the "personal scale," is to be distinguished from that of the "personal equation" [44] which is also associated with the

[41] Plassmann: Festschrift der Görres-Gesellschaft für Georg von Hertling. Kempten and München, 1913, S. 497 ff.; quoted by Bauch: Beobachtungsfehler in der Meteorologischen Praxis. *Fortschritte der Psychologie,* 1914, *2*:252.

[42] *Op. cit., p.* 286.

[43] Townley: Notes from the time-service of the Washburn Observatory. *Astronomy and Astro-physics,* Northfield, Minn., 1892, 11:467-9.

[44] An excellent historical and explanatory account of the Personal Equation has been written by E. C. Sanford: *American Journal of Psychology,* 1888, *2*:3-38, 271-298, 403-430.

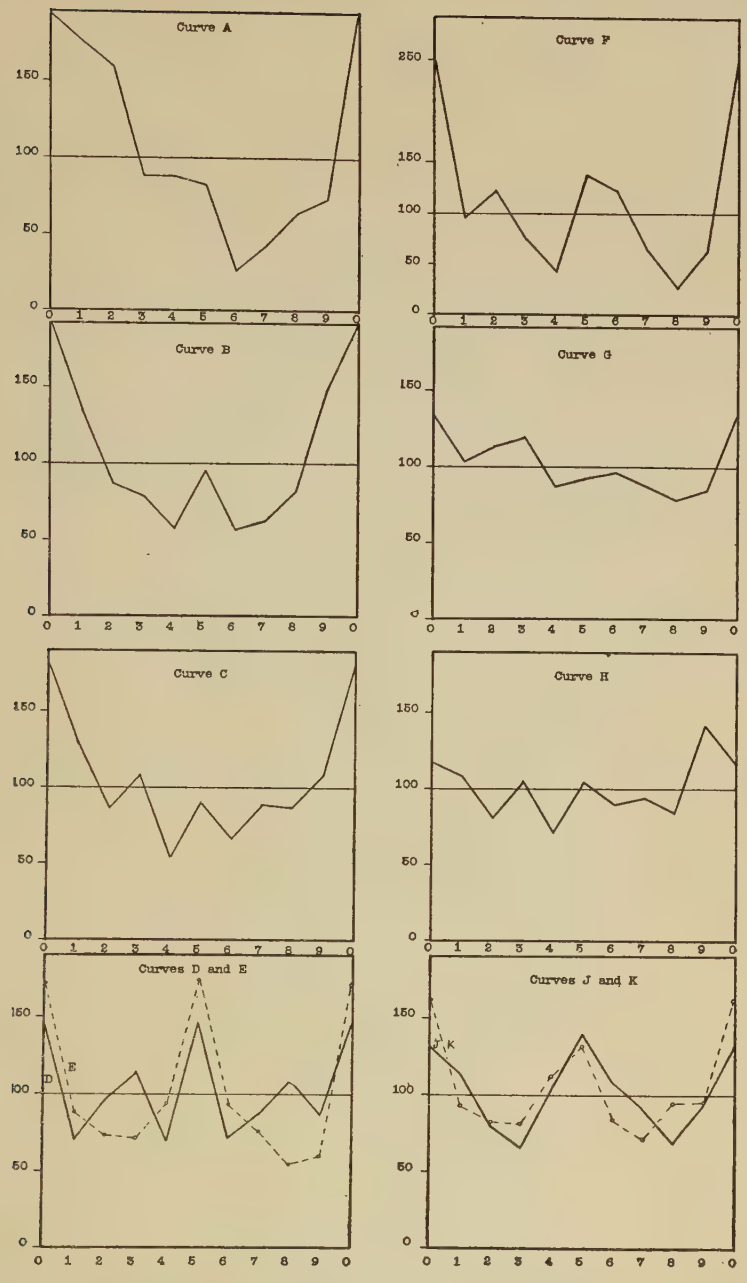

Plate XXVI.— Star-Transits (continued). Distributions showing independence of *"l'équation décimale"* of the special senses.

Curve					
A.	Paris Observatory, observer O.	Eye-and-ear.	1000 cases.	(From Boquet.)	
B.	" " " O.	Eye only (Cercle)	1000 "	"	"
C.	" " " O.	Eye only (Niveau)	1000 "	"	"
D.	" " " O.	Ear only (time)	1000 "	"	"
E.	Warendorf Gymnasium. Plassmann.	Ear only (time)	961 "	"	Bauch.
F.	Washburn Obs.	Eye-and-ear method. Ear only.	188 "	(From Townley.)	
G.	" "	Eye-and-ear method. Ear only.	816 "	"	"
H.	" "	Chronograph method. Eye only.	211 "	"	"

—Thermometric Observations, with .5° Calibrations. Showing curves comparable with curves *D* and *E* in which .5 sec. was the standard of judgment.

J.	Erfurt, 1901-10.	3652 cases.	(From Hellmann.)
K.	Wasserleben, 1901-10.	3652 cases.	" "

estimation of star-transits but which is more directly operative in the chronograph method of observation. By this method the observer with his hand on a telegraph key signals the moment the image of the star is bisected by the meridian line. His "personal equation" is his reaction-time. And since it, too, is subject to the influence of mental habit, even to the extreme of becoming a negative quantity (in anticipatory reaction), its operation in astronomical observation constitutes another distinct type of influence of mental habit upon judgment. Of course, the "personal equation" is also operative in the eye-and-ear method of observation, in fixing the locations of the star-image the distance between which the meridian intersects; and its effect may be simulated if the aggregate of the frequencies of the tenths *6-9* exceeds that of the frequencies of the tenths *1-4* in the observer's "personal scale," for, obviously, an average of a number of observations would be too large, the transit would be reported too late. On the other hand should the excess fall upon the tenths *1-4*, the usual "personal equation" would be opposed and the transit would be reported too early. But, practically, as was remarked by Chauvenet,[45] since the effect of the "personal scale" of an observer may be considered as constant, and as combining with the "personal equation," it may be regarded as forming part of the latter, as determined from a large number of observations. Corrective formulæ for the "personal equation" eliminate the effects of both from the mean of a large number of observations. For correction of the time of single transits, however, "personal scale" formulæ are required.

Estimates of Time and Space.

Some experimental results of estimates of time and space contribute to our inquiry concerning the influence of mental habit upon judgment.

Yerkes and Urban[46] had 525 reagents (251 young men from 17-23 years of age, and 274 young women from 17-20 years of age) estimate in seconds four different intervals of time (18, 36, 72, and 108 seconds) under four varying conditions (idleness, listening to reading, writing, and estimating by counting or otherwise). The men gave 4014 judgments; the women 4375. When the two sets of judgments were distributed over the digit-scale according to the unit's digit they gave the

[45] Chauvenet: A Manual of Spherical and Practical Astronomy. Philadelphia, 1891, vol. II, p. 193.

[46] Robert M. Yerkes and F. M. Urban: Time-estimation in its relation to sex, age, and physiological rhythms. *Harvard Psychological Studies*, 1906, *2*: 405-430.

distributions shown in Curves *A* and *B*, Plate XXVII. Part of the preference for *0* and *5*, however, was caused by a mental habit of estimating in simple fractional parts of a minute (15, 30, 45, 60 seconds). After the influence of this habit was eliminated [47] the distribution shown in Curve *C* was obtained. The deviations in this curve, then, show the influence of mental habit in the use of numbers when relatively long intervals of time are estimated.

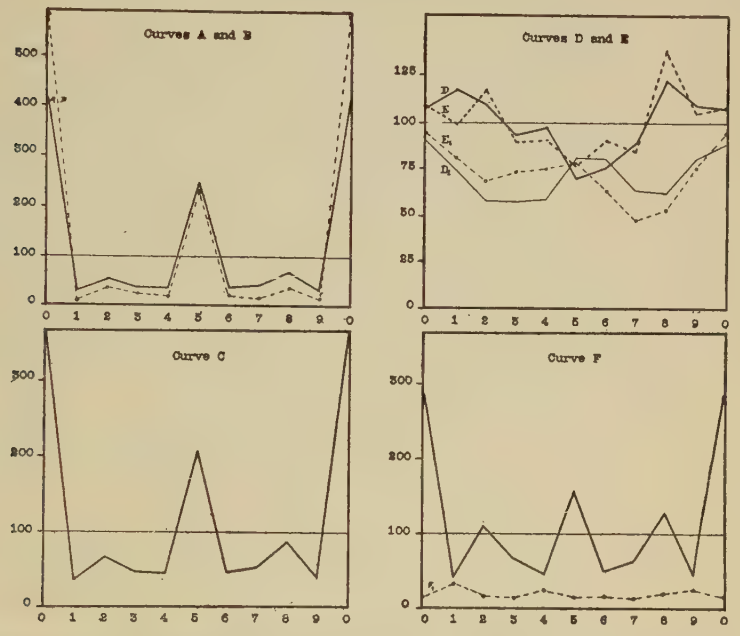

Plate XXVII.— Time and Space Estimations. Distributions showing number-preference and the "personal scale."

Curve *A*. Time estimates, men, 4014 judgments. (From Yerkes and Urban.)
B. " " women, 4375 " " " " "
C. " " Corrected Aggregate. " Urban.
D. Space estimates, $^1/_{10}$ millimeter, horizontal 3000 cases. (From Bauch.)
E. " " " " vertical. 3000 " " "
F. " " $^1/_{100}$ decimeter, horizontal. 1000 " " "

Michael Bauch [48] sought experimentally to determine the common "personal scale" in judgments of divisions of small and large spaces between calibrations. By exhibiting each division an equal number of

[47] *Vide*, F. M. Urban: On systematic errors in time-estimation. *American Journal of Psychology*, 1907, 18: 189.

[48] Bauch: Psychologische Untersuchungen über Beobachtungsfehler. *Fortschritte der Psychologie*, 1913, 1: 169-226.

times he could eliminate the error due to chance deviation which certainly is not avoided when the presentation of the various divisions is left to nature. Grossmann, for example, found,[49] by the use of *Mikroskopmikrometertrommeln* which permitted the reading of time to a hundredth of a second, that the distribution of 1458 transit-times over the tenth's digits presented deviations as high as ±8 per 1000; and of 2037 transit-times, as high as ±6.

Bauch presented each division with very accurate apparatus, 300 times, to 10 reagents who made 300 judgments each. Each distribution included 3000 judgments. Curve *D* shows the distribution of judgments of the decimal divisions of a millimeter when the scale was horizontal; Curve *E*, when it was vertical. Only two reagents were common to the two sets, and six out of the ten in the latter set were experienced meteorological observers. Curve *F* gives the distribution of 1000 judgments by ten other observers upon percentile divisions of a decimeter, expressed in centimeters and millimeters; the centimeters were disregarded in the distribution (only 14 in the aggregate of 1000 judgments were in error as much as a centimeter).

Whether the millimeter scale is horizontal or vertical the influence of mental habit in the estimates of decimal parts is present; although the observers have succeeded in reducing the usual preference for *0* they have not succeeded in avoiding a large negative error on *5*. And in the estimates of percentile parts of a decimeter (almost 4 inches) the estimate approaches more nearly a guess and falls more completely under the control of mental habit.

With respect to the ratio of correct judgments, it is interesting to note that for the horizontal millimeter-scale it was 71%, for the vertical millimeter-scale, 70.5%, and for the decimeter-scale, only 18%; and that in the estimates of the decimal parts of a millimeter the R cases are higher (in per cent of guesses delivered) upon the *0* and *5* tenths (see Curves D_1 and E_1), while in the guessing of the percentile divisions of a decimeter, they are higher on *1*, *4*, and *9* (see Curve F_1),—the millimeter numbers least often guessed,—which indicates that for these values the judgment was more often an *estimate;* that is, in the course of judging, the influence of mental habit determined a relatively greater number of *guesses* upon the digits *0*, *5*, *2*, and *8*, just as it did in the giving of ages and of students' grades.

The close agreement of Curves *C* and *F*, the one resulting from estimates of relatively long time, the other from estimates of a relatively

[49] *Op. cit.*, p. 151.

large space, with Curve *C*, in Plate XX*a*, (star magnitudes; Grossmann's Observer *H*.); with Curve *B*, Plate XXI (temperature in Mauritius); Curve *B* (New England rain-fall), Curve *E* (estimates of age), Curve *F* (terms of criminal sentence), Curve *G* (star-magnitudes), and Curve *H* (students' grades), in Plate XXII, as well as with Curve *D* (ages of the Latin dead), Plate XXIV, indicates quite clearly that we have here the typical form of a "personal scale" which results from the influence of mental habit upon judgment when judgment is delivered upon such values as permit a fairly wide range of error.

Thus far, the estimates which reveal the influence of the number-habit were made upon unknown quantities under such conditions that they could be expected to approximate more or less closely the objective values of those quantities. This restriction in the range of error, it would seem, should limit considerably the influence of number-habits, unless the very limitation emphasizes in the mind the unit marks on the scale used in making the estimate. Some experimental results are available for determining the extent of the influence of mental-habit upon estimates when their approximation to the objective values could not be expected to be close; *i. e.*, when the range of error was large. Let us examine several studies in guessing.

Studies in Guessing.

Professor Dresslar[50] examined the returns of a guessing contest held in a clothing store in Los Angeles, California. A prize of one hundred dollars in gold was to be given to the person or persons who guessed the number of seeds in an uncut monster squash which was on exhibition. A guess was made by each one of 7,700 persons. Many of the guesses were as high as 1,000,000, while several were more than 10,000,000; 6,863 (4,238 made by men, 2,625 by women) fell below 1000, and these were selected for the study.

The prize was divided among three contestants each of whom guessed 811, declared by the officials to be the true count.

Professor Dresslar said:

> It occurred to me that a study of these guesses would reveal some interesting number preferences, if any existed, for the conditions were unusually favorable for calling forth naïve and spontaneous results, there being no way of approximating the number of seeds by calculation, and very little or no definite experience upon which to rely for guidance. It seemed probable, therefore, that the guesses would cover a wide range, and by reason of this furnish evidence of

[50] Dresslar: Guessing, as influenced by number preferences. *Pop. Sci. Mo.*, 1899, 54: 781-6.

whatever number preference might exist. It is undoubtedly safe to assume, too, that the guesses made were honest attempts to state as nearly as possible best judgments under conditions given; but even if some of the guesses were more or less facetiously made, the data would be equally valuable for the main purpose in hand.

According to the theory of probability, had there been no preference at all for certain digits or certain combinations of digits within the limits of the guesses, one figure would occur about as often as another in units' or tens' place. It was argued, therefore, that any marked or persistent variation from such regularity in such a great number of cases would reveal what might be termed an unconscious preference for such numbers or digits for these places.[51]

If the digits occupying the unit's place, in the respective guesses, are tabulated over the ten-digit series, we get the distribution shown in Curve *A*, Plate XXVIII; the distribution of the digits occupying the ten's place is shown in Curve *B*.

Professor Sanford,[53] suspecting that a thorough study of guessing might "be expected to throw light upon some of the less obvious, and

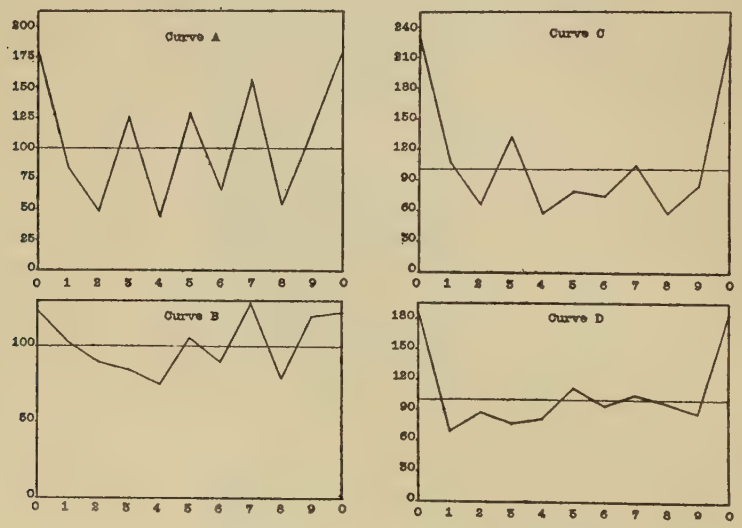

Plate XXVIII.— Guessing. Curves showing number-preferences.

Curve *A*. Guessing of seeds in a squash, unit's digit. 6863 cases. (From Dresslar.)
B. " " " ten's " 6863 " " "
C. " beans " bottle, unit's " 1043 " " Sanford
D. " " " ten's " 1043 " " "

[51] *Loc. cit.*, pp. 781-2.

[53] Sanford: On the guessing of numbers. *Am. Jr. Psychology*, 1903, 14: 647-665.

perhaps unconscious, influences that determine opinion and action" (p. 645), made a study of the returns of a guessing contest held by a dealer in photographic supplies, in Worcester, Massachusetts; 2817 guesses were made by 767 persons on the number of small white beans contained in a 5-pint bottle. The range in the 2,573 guesses of the 651 men was from 285 to 3,425,602; in the 244 guesses of the 114 women, from 250 to 2,675,181,756. After the elimination of the frequent guessers (those who guessed more than five times), and of a few irregular guesses, there remained, for the purpose of the study, 1043 guesses, with a range of 285 to 1,000,000, and a median of 7,257. The correct number of beans was announced to be 8,834. Tabulation of the digits occupying the unit's place gave the distribution shown in Curve *C*, Plate XXVIII; of the digits occupying the ten's place gave the distribution shown in Curve *D*.

It will be noticed that these curves bear a strong family resemblance to the curves of Plate XXII,[54] which exhibit the distributions of estimates of such values as preclude a wide range of error. The round-number preference is very marked on the *0* abscissa. The preference for *5* is overshadowed by stronger preferences for *3* or *7*. Dresslar deduced from his study the following preferences, which he suggested may be widely habitual:

1. For *0* (round numbers).
2. For odd numbers, especially *7*.
3. For duplication; such as *777*.

Sanford tabulated the following preferences:

1. For round numbers.
2. For particularized numbers; as, *7001, 4035 1/2*.
3. For repetitional numbers (Dresslar's duplication).
4. For symmetrical numbers; as, *10101*.
5. For serial numbers; as, *1234, 9876, 6783*.
6. For particular digits.[55]

He pointed out that

> . . . Number preferences—so far at least as they can be judged by mass returns—are not constant, but vary with the conditions under which the numbers are used. The odd numbers are preferred in the unit's place in "guessing contests," but the even (next after the 5's and 10's) in the estimations of ages, and two years is the most frequent criminal sentence.[56]

[54] *Supra*, p. 253.

[55] The order of preference, in unit's place, was 0, 3, 1, 7, 9, 5, 6, 2, 8, 4.

[56] *Op. cit.*, p. 398.

And he concludes his study with the following summary:

> The data presented seem to me to bring out clearly several points with regard to habits in the guessing of numbers. (1) These habits are not fixed and constant, as seems generally to have been assumed, but vary characteristically with variations in the conditions under which the guessing is carried out. (2) Two-thirds of the guessers in the "guessing contest" here studied made use of particularized numbers, showing more or less preference for certain digits, especially in the unit's place. (3) About one-third guessed round numbers or those adjacent to them, or numbers showing a repetitional or serial character in the digits chosen. There was also a slight, but uncertain, indication of a more general tendency to move, in choosing a series of digits, by short steps along the digit scale. (4) No evidence was discovered that the guessing habits of women and girls differed from those of men and boys. (p. 665).

Sanford's conclusion that number-preferences vary with the conditions under which the numbers are used, is important. He was assisted to this conclusion by a comparison of his results with those of Professor Minot who collected from 10 persons 8,600 guesses on the first ten numbers, *0-9*.

The study made by Professor Minot[57] relates the number-habit directly to experiments in thought-transference, carried out under the direction of the Committee of the old American Society for Psychical Research. The instructions (*vide, Proceedings,* Series I, 1:15-6) provided for the guessing of the digits from *0* to *9* which were to be entered on a blank in irregular order; the guessing to progress in series of ten in direct and reverse directions over the recorded order. The agent, of course, who determined the order of the digits, entertained in his mind the proper digit during the forming of each guess by the percipient. But the latter, knowing the conditions of the experiment, would have a strong incentive to distribute his guesses quite evenly over the number series; any marked deviations, in a considerable number of guesses, would indicate an unconscious influence of number-habit.

Curves *A, B,* and *C* in Plate XXIX show the distributions of three respective percipients who made 1000 guesses each; and Curve *D* shows the distribution of the aggregate of 8600 cases.

It is seen at once that, although there is some individual variation in number preferences, the type of curve is different from the type displayed in all of the preceding plates.

In our own experiments on thought-transference,[58] when the card

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[57] Minot: The Number-Habit. *Proceedings Am. S.P.R.,* 1886, Series I, 1:86-95.

[58] *Supra,* pp. 48 ff.

was drawn by chance and the guesses included the number of spots from *1* to *10*, the conditions were not so favorable for an equitable distribution of the guesses over the respective numbers, although the reagent (percipient) knew that in the long run the respective numbers must be drawn an approximately equal number of times. On the whole the conditions were not very different from those of the American Committee. Each reagent made 100 guesses. Curve *E* gives the distribution for an aggregate of 10,000 guesses.

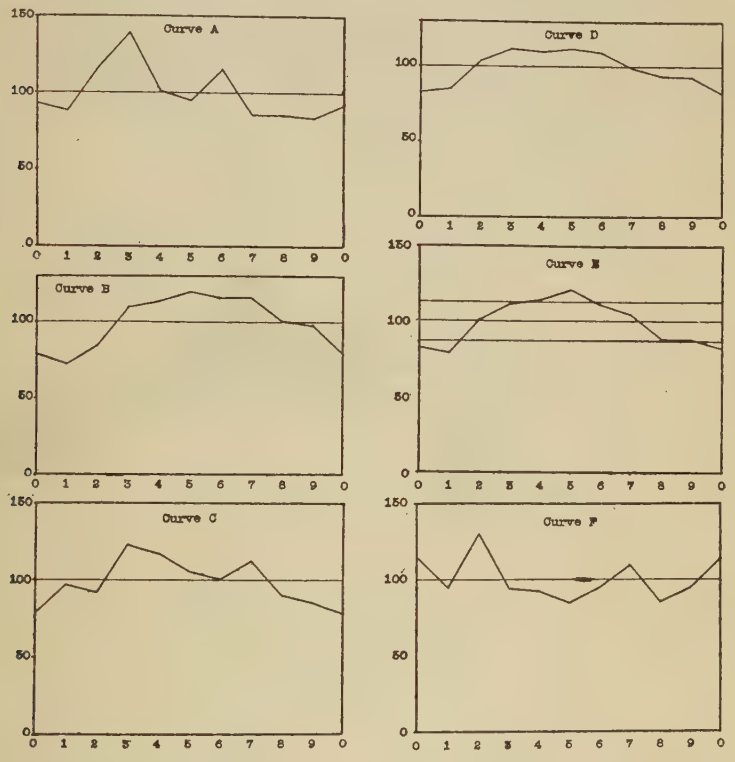

Plate XXIX.— Guessing (continued). Curves showing a new type of "personal scale."

Curve						
A.	Guessing of digits 0-9.	Reagent G. B.	1000 cases.	(From Minot.)		
B.	" " " "	G. W. N.	1000 "	"	"	
C.	" " " "	C. H. B.	1000 "	"	"	
D.	" " "	Aggregate, 10 reagents.	8600 "	"	"	
E.	" of playing-cards (1-10).	100 reagents.	10,000 "		(Stanford.)	
F.	" " lotto-blocks (10-90).	H. Unit's digits.	1000 "		"	

The preferences shown in these curves are for intermediate digits, and are to this extent controlled by the special conditions of guessing which encouraged an unconscious tendency to pass more or less irregularly up and down the scale of digits, accumulating guesses on the intermediate digits and relatively ignoring the terminal digits.

The consistency of the curves presented above may be readily accepted as evidence that their general form is not due to chance deviation. Were statistical evidence desired, however, it could be readily supplied; *e. g.*, in Curve *E* of Plate XXIX, the number of guesses plotted is 10,000,

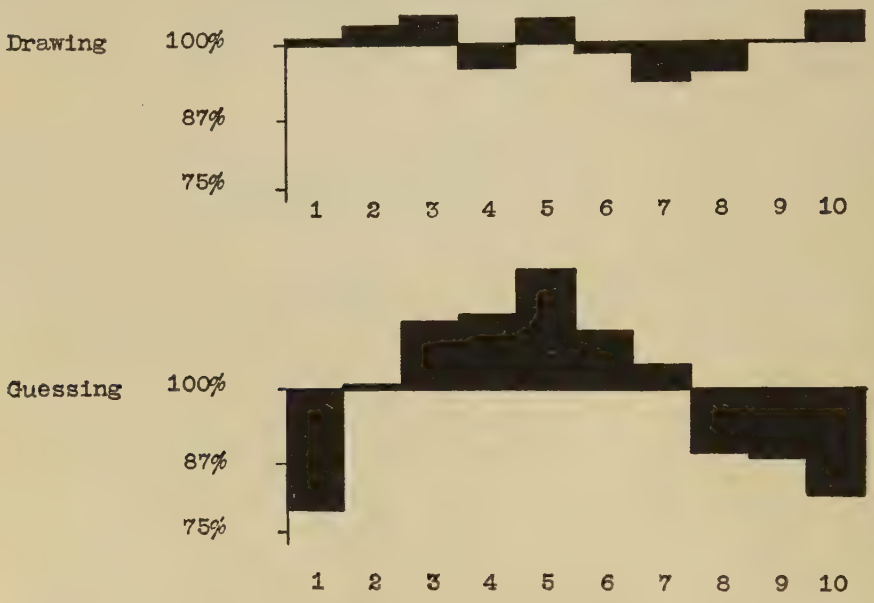

Plate XXX.— Deviations from Probability in the frequency the respective cards (spots 1-10) were *drawn* and *guessed*. Showing influence of mental habit in guessing. 100 Reagents. 10,000 cases.

and the probability is 10%, or 100 per M. From Table XLI [59] we find that the limit of chance deviation is 1.3% or 13 per M; any values lying outside the field of 87-113 are consequently indicative of extra-chance cause. These upper and lower limits of chance deviation are shown in the curve, and four of the ten values are seen to lie definitely beyond these limits, while four more very closely approach them. This evidence is corroborated by the excess of deviation in the guessing over the deviation in the drawing as shown in Plate XXX, which is drawn from the card-guessing data. The upper curve shows the deviations resulting from drawing the

[59] *Supra*, p. 89.

cards by chance from a shuffled pack, and constitutes "empirical probability."

Distribution of the Number-Habit.

Whether the influence of mental habit upon judgment, as shown in the curves which have been displayed in the above pages, is shared by only a relatively few individuals whom the influence dominates to a high degree, or by the great majority of the persons contributing the data whom the influence dominates in a lower degree, is a question that may be inspected a little more narrowly. That the distribution of the number-habit is fairly general is indicated by several facts already before the reader: (1) The data contributed by a large number of people (as in age-returns, students' grades, and the guessing of the number of seeds in a squash, or of the number of pips on a card) show number-preferences in large amounts; and (2) the data contributed by individual scientific men do not fail to show the influence of mental habit. The results of our experiments in guessing, however, throw some more definite light on the question.

Individual variation, as has already been observed, must be expected here. In Plate XXII [60] Curves *C* and *D* show some individual variation between two observers in scientific estimates, of the same kind; notably, on abscissa 5. In Plate XXIX,[61] Curves *A, B, C,* and *F* [62] show individual variation in the number preferences of four reagents in guessing the ten digits. In Curves *A, B,* and *C, 3* is preferred; in *B, C,* and *F, 7*; in *A* and *B, 6*; in *A* and *F, 2*; and in *B, 5*. Since these curves are based upon 1000 guesses each, the indications of preference just noted may be regarded as fairly reliable.

In our own card-guessing, since each reagent made but 100 guesses, the individual curves are not so reliable criteria of individual preferences. But if they are compared *en masse* with the corresponding curves of cards drawn, *i. e.*, the empirical probability curves, the fact of fairly liberal distribution of number-preference may be appreciated by visual inspection. Plate XXXI presents in greatly reduced size both classes of curves for our 100 sets of 100 experiments each. It will be seen that some of the number preferences (in the lower half of the plate) are suf-

[60] *Supra,* p. 253.

[61] *Supra,* p. 269.

[62] This curve gives the 1000 guesses of the digit in unit's place made by the reagent whose mental habit in Lotto-Block-guessing was displayed in Plate II, p. 44. It is not quite comparable with the other curves drawn from the guesses of only the ten digits.

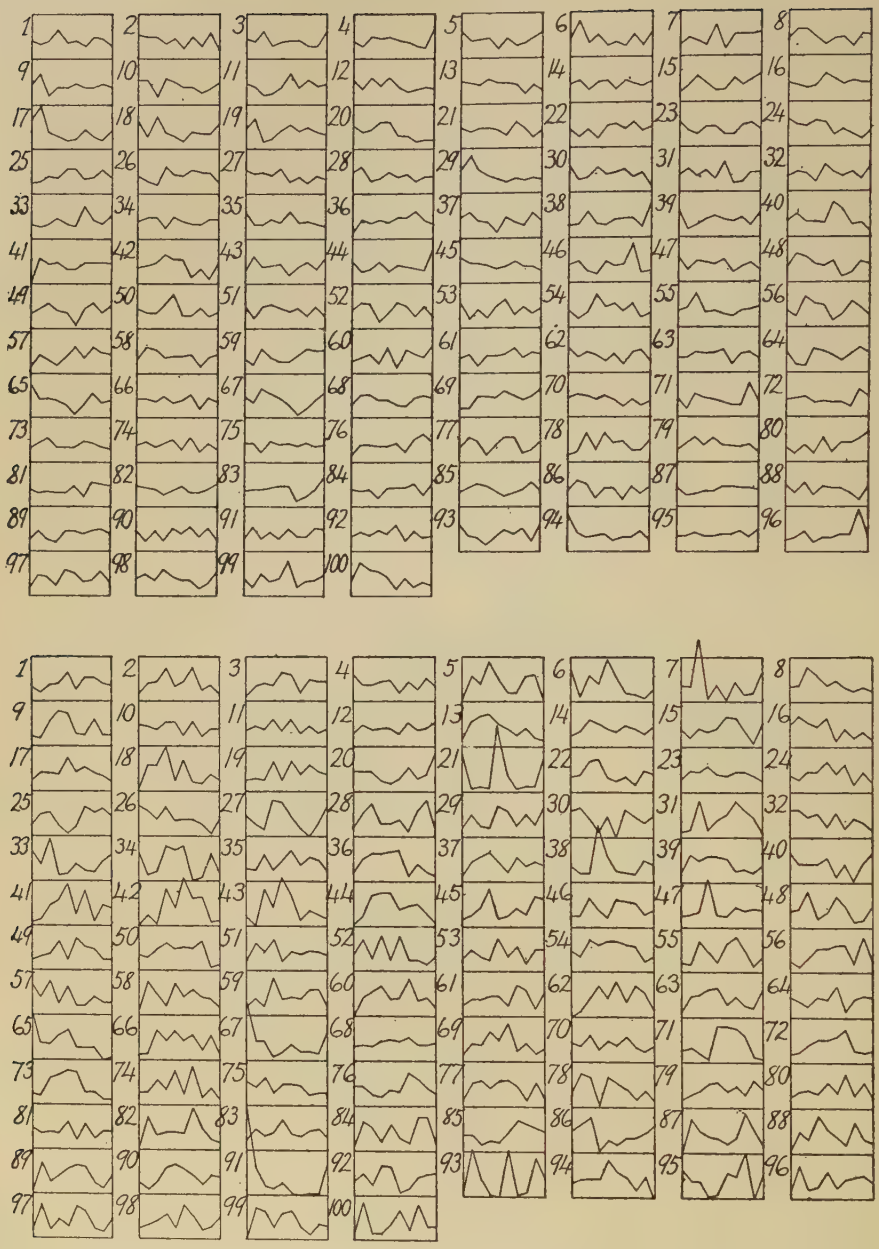

Plate XXXI.— Frequency of Occurrence of the respective cards (spots 1-10) in sets of 100 experiments, as drawn (upper series) and as guessed (lower series). Showing distribution of mental habit among the 100 reagents.

ficiently great to be recognized and definitely identified; *e. g.*, those shown in the curves of Reagents *7, 18, 21, 38, 42, 43, 47, 65, 67, 91, 93,* and *95*. In these cases the curves touch or cross the upper enclosing line, and thus exceed the largest deviations, shown in the curves of drawn numbers (in the upper half of the plate), namely, *17* and *96*. Moreover, four types of curves may be distinguished: The "U" type, as *21, 67,* and *91*, showing preferences for the terminal digits *1* and *10*; its reverse, as *5, 13, 17, 18, 31, 36, 41, 42, 43, 44, 49, 50, 55, 56, 58, 60, 62, 66, 69, 71, 72, 73,* and *90*, showing a general preference for the intermediate digits; the much serrated curve, as *2, 27, 34, 48, 52, 57, 74, 87, 88, 93, 97, 100*, showing two or more strong preferences for digits not grouped

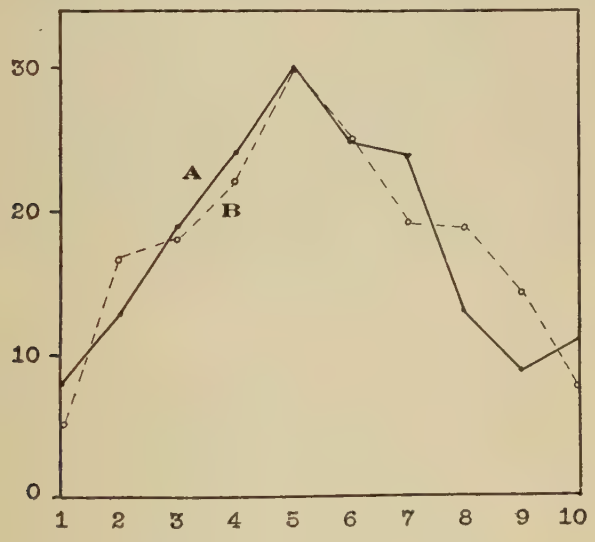

Plate XXXII.— Number-Preferences.

Curve *A*. Distribution of the 176 stronger preferences in our card-guessing.
B. " " preferred unit's digits. 469 German reagents. 2346 choices. (From Marbe.)

together; and the single strong preferences, as *7, 38,* and *47*. The individual preference may be for any of the digits; Reagent *91* prefers 1; *78*, 2; *7*, 3; *38*, 4; *21*, 5; *42*, 6; *82*, 7; *95*, 8; *28*, 9; and *20*, 10. If we tabulate the 176 stronger preferences upon the digit series we get the distribution shown by Curve A, Plate XXXII. The excess of preferences for *5* and its neighboring digits is in agreement with the results of an investigation of number-preference conducted by Karl Marbe,[63] and shown

[63] Marbe: Ueber das Gedankenlesen und die Gleichförmigkeit des psychischen Geschehens. *Zeitschrift für Psychologie,* 1910, 56:253-5.

in Curve *B*. In Frankfurt 308 school girls (7-17 years of age), and in Würzburg 161 persons (seminary students and soldiers) were required to write down as quickly as possible any number they pleased (eine beliebige Zahl) from 1 to 10, 11 to 20, 21 to 30, 31 to 40, and 41 to 50.

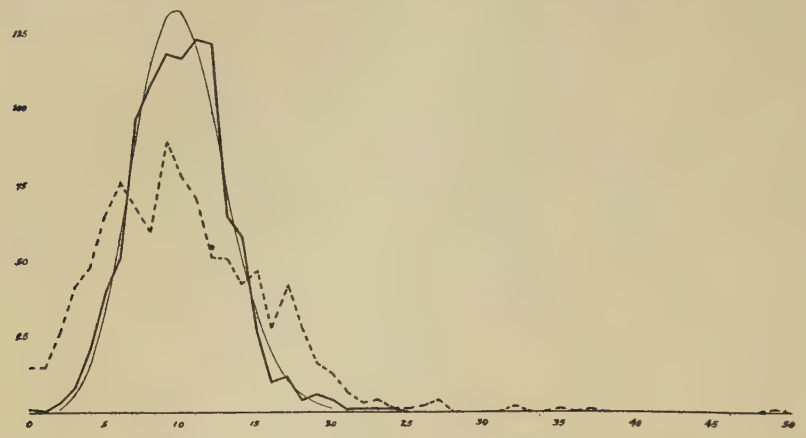

Plate XXXIII.— Distribution of the Number of Times a *number* was drawn (solid line) or guessed (broken line) in 100 experiments. The theoretically probable distribution is shown by the smooth curve.
100 reagents. 1000 cases.

Curve *B* gives the aggregate distribution for the 2346 choices of the unit's digit by the 469 reagents.

If we disregard what the particular preferences are and seek to determine more definitely to what extent they have played a part in the guessing, we can tabulate the number of times each reagent has guessed each of the ten digits. If his guessing were distributed evenly, each digit would be guessed just ten times; if distributed by chance, the deviations from 10 times would be distributed about 10, in such a way as to conform to the empirical distribution (of the drawings) or to the theoretical distribution; if, however, the number-habit played an appreciable rôle, the deviations would be more widely distributed than in either of the other distributions. If then all of these values, for the 100 reagents, are aggregated, a definite spread of the distribution beyond the two chance-distributions would indicate that the guessing of an important part of the reagents was influenced by the number-habit, since a single reagent contributes but ten values to the aggregate of 1000 values. Plate XXXIII displays the three distribution-curves. The abscissæ (at the bottom of the plate) denote the number of times a digit (any digit) was guessed by a reagent (any reagent) in his hundred experiments. The ordinates (on the left margin) indicate the number of cases (or the frequency) of the abscissal values. The distribution-curve of the guesses shows a

definite spread beyond the chance distribution-curves. There are 15 cases when a digit was not guessed at all in a hundred guesses; 42 cases a digit was guessed but 3 times, while chance calls for but 5 or 8 cases; 48 cases of 4 times, chance calling for but 15 to 22 cases; 42 cases of 17 times, chance calling for but 12 cases; etc., and, to compensate, the more central values (7-14 times) occur much less frequently than chance calls for. On the lower (left) side of the curve of guesses there are 152 cases lying outside the range of chance deviation; and on the upper (right) side, 113; making a total of 265, or 26.5%, of the guesses definitely controlled by mental habit. The reagents not only preferred certain numbers; they avoided or ignored certain numbers even to a greater extent. If mental habit influenced guesses of individual reagents on half of the ten digits, 53% of the reagents would be necessary to contribute 265 cases; or if on three of the ten digits, 88% of the reagents would be necessary.

That the proportion of reagents whose guesses were influenced by mental habit must be large, and also that the respective mental habits must be largely common, may be seen from Plate XXXIV, which gives the distribution-curves of the "number of times a digit was guessed in 1000 experiments by 10 reagents." Since there were ten groups of 10

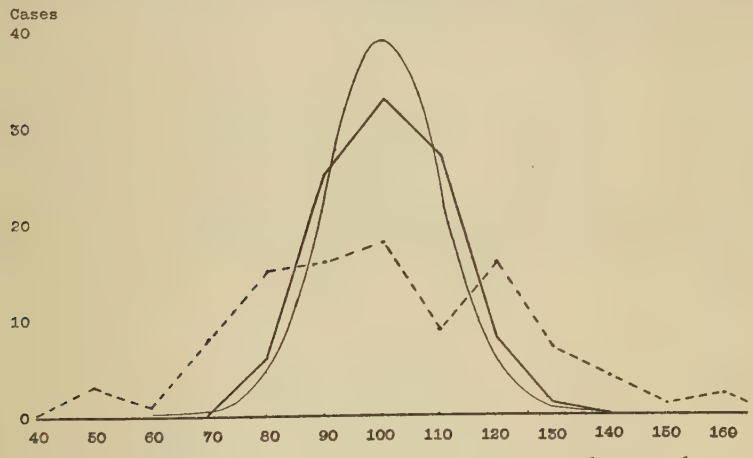

Plate XXXIV.— Distribution of the Number of Times a *number* was drawn (solid line) or guessed (broken line) in 1000 experiments by 10 reagents.
10 groups of 10 reagents. 100 cases.
The probable distribution is shown by the smooth curve.

reagents, each of which contributes 10 values, the curves are plotted from 100 values. The central value is 100 times. On the central abscissa, however, the values of 96-104 are plotted; thus each abscissal value is the central of ten aggregated inter-class values. The spread of the curve

of guesses beyond the chance-curves is even more marked than in the preceding Plate. The number-habit may be regarded as fairly general among our 100 reagents.

Although the number-habit is fairly generally distributed among our reagents, Plate XXXI [64] shows that there were individual differences not only in the controlling force of the habit but also, as was noted above, in the preferences; each digit being markedly preferred by some reagents, but the intermediate digits being more frequently favored, as was shown in Plate XXXII. This sort of variation tends to erase indications of the influence of number-preference when the guesses of all the reagents on the respective digits are aggregated, consequently the curves shown in Plates XXIX (Curve *E*) and XXX, above, do not show the full force of the influence.

More agreement is found in the habit of neglect which more often concerns the terminal digits. This trait in guessing, it may be recalled, was operative in the judgments delivered by our reagents in the experiments on the influence of peripheral subliminal impression, to such a degree that the probability from which significant deviations were sought had to be revised in accordance with the limitations which 19 reagents out of 26 had imposed upon themselves.[65]

Other Mental Habits.

Our experiments in card-guessing permitted the appearance of other mental-habits for which we may search in our data. Was preference shown for color, for suit, or for particular cards?

Red cards were drawn 4894 times in the 10,000 experiments, and red

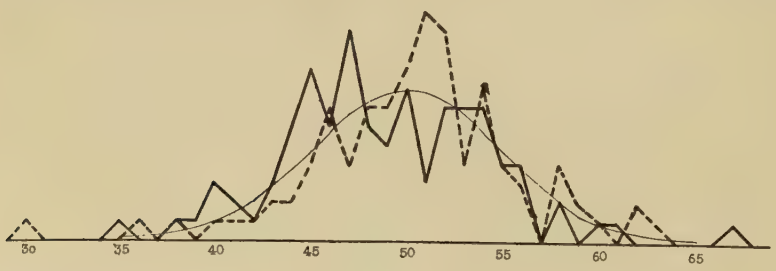

Plate XXXV.— Distribution of the Number of Times a *red card* was drawn (solid line) or guessed (broken line), in 100 experiments. 100 reagents. 100 cases. Theoretical probability is represented by the smooth curve.

[64] *Supra*, p. 272

[65] *Vide, supra*, pp. 207 f.

cards were guessed 5043 times. This approximation to theoretical probability (5000) is remarkable, considering that chance permits a deviation of 210.[66] If there are any individual preferences they are completely canceled in the aggregated results. That individual preferences were negligible may be seen from Plate XXXV, in which the distribution-curve of guesses may be compared with the curves of drawings and of theoretical probability. Although both empirical curves are much serrated they follow the general course of the theoretical curve. There were only three cases where red cards were guessed by an individual reagent over 62 times in his hundred guesses; and only one case below 35, which, however, since it is so far below the lower limit of the chance distributions, indicates a significant avoidance of red cards on the part of one reagent. But this does not indicate color preference; the reagent (7) had a preference for Spades and neglected Diamonds, Hearts and Clubs being guessed equally often.

Plate XXXVI shows the deviations in the drawing and the guessing of suits, for the aggregated results. Hearts were guessed 2654 times

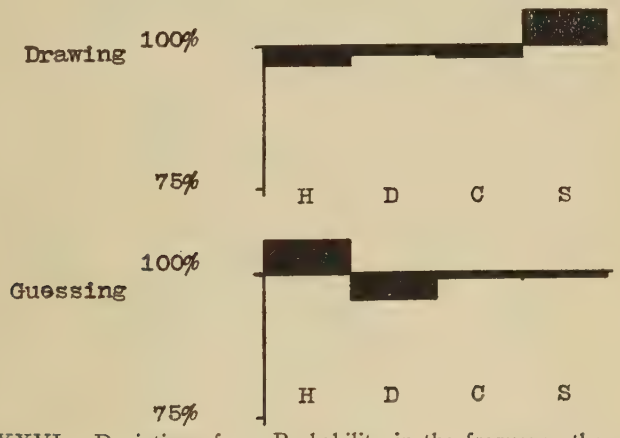

Plate XXXVI.— Deviations from Probability in the frequency the respective *suits* were drawn and guessed. 100 reagents. 10,000 cases.

while the most probable number is 2500 times, and Spades were drawn 2660 times. Since chance allows a deviation of 180,[67] or an aggregate of 2680 times, we have no indication of general suit-preference. A few individual reagents, however, were subject to mental habit with respect to suit, as may be seen from Plate XXXVII, which gives the distribu-

[66] *Cf.*, Table XLI, p. 89.
[67] *Cf.*, Table XLI, p. 89.

tions of the number of times a suit (any suit) was guessed, was drawn, and is to be expected, in a set of 100 experiments. The distribution-curve of the guesses (broken line) is sensibly lower for the central values than the chance-curves, and its extreme values extend beyond the chance limits. There are 10 cases where a suit was guessed less than 14 times, and 5 cases more than 39 times: Two reagents preferred Hearts; two, Spades; and one, Diamonds; three of these five reagents avoided a suit, (two, Clubs; one, Diamonds); and among the seven remaining reagents who avoided a suit four avoided Diamonds; two, Clubs; and one,

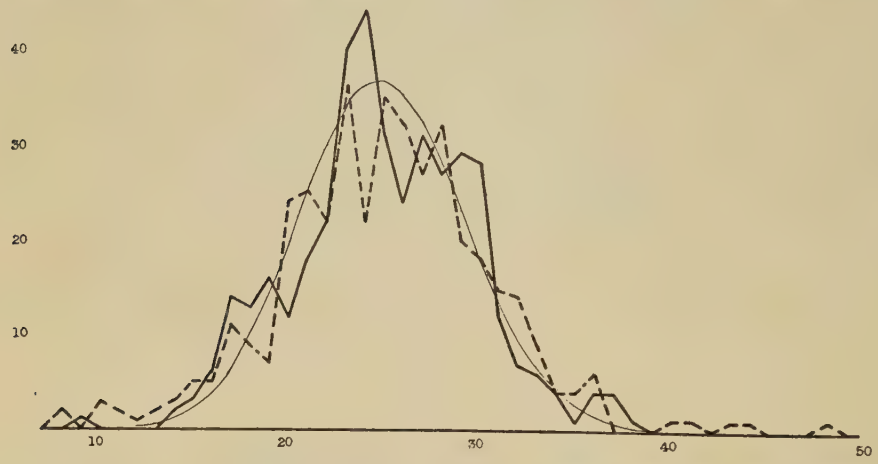

Plate XXXVII.— Distribution of the Number of Times a *suit* was drawn (solid line) or guessed (broken line) in 100 experiments. Showing influence of mental habit. 100 reagents. 400 cases.
Theoretical probability is represented by the smooth curve.

Hearts. Were these few cases to be accepted as indicative of a general mental habit, one would say that Hearts and Spades are preferred, Diamonds and Clubs are avoided. But such a mental habit is not general; it influenced to a definite degree only 12% of our 100 reagents in their guessing of suit.

Mental habit also influenced the guessing of individual cards; and since aggregate guesses upon the individual cards show its influence clearly, many of the preferences must be common. As may be seen from Plate XXXVIII, which presents the deviations in both drawing and guessing from the most probable number (100%), the deviations are definitely greater in the guessing than in the drawing. Accepting the latter as empirical probability, deviations greater than 20% of the most probable number (made on the Five of Hearts) indicate some extra-

chance cause. According to Table XLI,[68] the limit of chance is .66 of the number of experiments, or 26.4% of the most probable number. It is certain that the Four and the Five of Hearts have been preferred and that the Ace of Clubs and the Ten of Spades have been avoided, by reason of mental habit; and it is more than probable that the general habit of favoring the intermediate digits and avoiding the terminal digits has influenced the guessing of the ten cards in each of the four suits, as may be seen in the approximate conformity of deviations on the ten cards under the respective suits. Much of the general preference for particular cards must therefore be attributed to the general preference for the intermediate digits.

There was, however, some influence of mental habit upon the guessing of particular cards. If we tabulate the number of times each of the

Plate XXXVIII.— Deviations from Probability in the frequency the respective *individual cards* were drawn and guessed. 100 reagents. 10,000 cases.

100 reagents guessed each of the forty cards, and aggregate all the values, we get the distribution shown (by the broken line) in Plate XXXIX, which also presents the distribution-curves of the drawing and of theoretical probability. The curve of guesses spreads beyond the curves of empirical and theoretical probability. About twice as many

[68] *Supra*, p. 89.

cards were avoided altogether (abscissa *o*) as chance allows; and many more cards were guessed over five times in 100 guesses than one would expect by chance. One reagent guessed the Ace of Clubs 17 times and the Ace of Diamonds 19 times,—making over a third of his 100 guesses on but two of the forty cards; another guessed the Six of Clubs 19 times and the Nine of Hearts 11 times; one guessed the Three of Spades 18 times, and another the Four of Spades 15 times. There were twenty-five cases of guessing a card 10 or more times. Of these, the Ace of Diamonds, the Three of Spades, and the Six of Clubs were preferred by three reagents; the Ace of Hearts, the Three of Hearts, the Six of Diamonds, and the Four of Spades were preferred by two reagents; the rest of the preferences were individual cards of all suits. There is almost no general agreement in the preferences; but, as may be estimated from the distribution-curves, the number of guesses controlled by mental habit

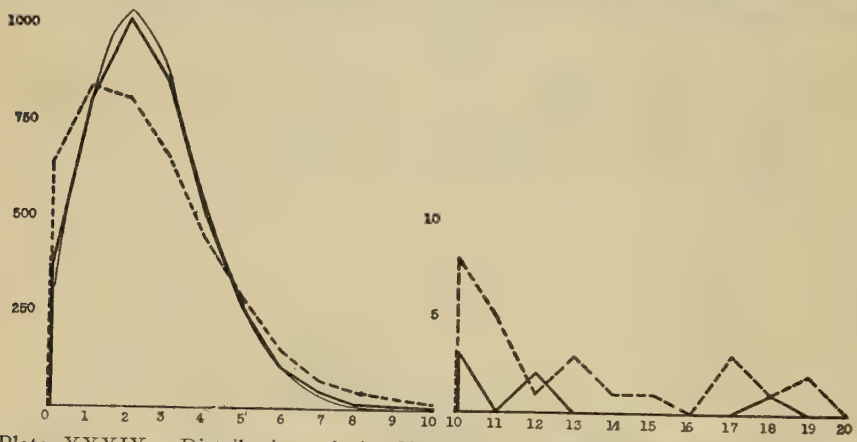

Plate XXXIX.— Distribution of the Number of Times an *individual card* was drawn (solid line) or guessed (broken line) in 100 experiments.
100 reagents. 4000 cases.
The theoretically probable distribution is shown by the smooth curve.

is so large that almost all of the reagents would be necessary to contribute them. Examination of the tabulations of the guesses of the individual reagents verifies this deduction. Mental habit was general.

Mental habit also influences the guessing of letters. In some experiments conducted by the writer several years ago with the tachistoscope, seven reagents failed to perceive a letter (a consonant) shown among others and were obliged to guess 380 times. The average number of guesses per letter would be 18; but *Q*, *W*, and *Z* were not guessed over 10 times, while *R*, *S*, and *T* were each guessed 28 or 29 times.

Each of three of the reagents failed to guess two letters and two avoided a single letter. Individual preferences here also varied greatly: One reagent preferred *D*, one *H*, one *K*, one *N*; two reagents preferred *L*, two *R*, two *S*, and two *T*.

A few other mental habits which influence judgment are worthy of notice here, especially since their operation is clearly unconscious and involuntary. They are the "constant errors" found, and reckoned with, in pyschological investigation. We can illustrate them by selecting but one type of experiment,—that of "stimulus comparison." That such "errors" may have practical importance beyond the psychological laboratory is seen from the early difficulties encountered by the astronomer when he sought by photometric methods to determine the relative brightness of the stars.[69] Many observations had to be discarded because it was found that it makes a difference to the observer whether the image of the estimated star is to the right or to the left of the image of the Pole-star. Correction was made by duplicating the observations after the two images were reversed in their relative positions. This is the so-called "space-error." When the two stimuli are compared not simultaneously, but one after the other, there is a corresponding "time-error." Assuming that the "Method of Right and Wrong Cases, or Constant Differences," is employed in stimulus comparison, there will be fewer right judgments if the norm comes after the variable—this error is called the "general tendency of judgment." Then, with a given temporal order of norm and variable, for some observers there will be fewer right judgments when the norm is greater than the variable, and for others when it is less than the variable—the so-called influence of "type." [70] Without mentioning an equal number of the more variable "errors," we may, perhaps, close this passing reference with the assurance that the reader will appreciate the fact that these "errors" which tax the ingenuity of the psychologists to eliminate, are *unconscious* influences upon judgment.

Explanatory Considerations.

Although it is of but secondary interest to know *why* we have mental habits after we know *that* we have them, a brief consideration of some of their causes may add somewhat to the cogency of the evidence ad-

[69] *Vide, Annals of the Astronomical Observatory at Harvard College,* 11:222; 14:4, 6.

[70] For a good illustration of these errors, *vide,* F. Angell's review of "Zur Analyse der Unterschiedsempfindlichkeit von Lillien J. Martin und G. E. Müller, Leipzig, 1899," in the *Am. Jr. Psychology,* 1900, 11:266-7.

duced to establish them, by way of making them more reasonable than they appear to the layman to be.

Professor Dresslar, in his investigation [71] with 875 young men and women who were attending the State Normal schools in preparation for teaching in the schools of California, conducted for the purpose of learning the extent of their belief in superstitions, found in the potent superstitions connected with numbers so great a preference for the odd numbers, particularly *3* and *7*, that he was constrained to say:

> These figures lead us to expect to find more than 80% of all superstitions, referring to numbers, making use of 3, 7, 9, or 13. These, then, can with propriety be designated as the numbers especially appropriated by the mind to express and embody superstitious notions. (p. 195).

And the original choice of these numbers he ascribes to a natural mental bias:

> The general unconscious preference for odd numbers [72] is a mental bias developed out of conditions imposed upon external nature by the force of gravitation. (p. 204).

In order for a free physical body to maintain a stable position it must have at least three points of support. In the natural process of deductive reasoning the major and minor premises either hold the judgment in suspense or lead directly to a conclusion (the third component of the syllogism). The number *3* becomes a satisfying numerical concept.

Professor Sanford says:

> An explanation of number-preferences, if one is attempted, must take several things into account. First and most important of these is that number preferences —so far at least as they can be judged by mass returns—are not constant, but vary with the conditions under which the numbers are used. The odd numbers are preferred in the unit's place in "guessing contests," but the even (next after the 5's and 10's) in the estimation of ages, and two years is the most frequent criminal sentence. Under some conditions the landmarks of the decimal system (5, 10, 15, 20, etc.) would be prominent; under others those of the duodecimal system [*e. g.*, criminal sentences in months].[73] . . . Number preferences should be explained, therefore, in connection with the special circumstances under which they are exhibited. (p. 398-9).

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[71] Fletcher B. Dresslar: Superstition and Education. Berkeley (California), University Press, 1907. (There was full or partial belief in 45% of the 7176 recorded superstitions.)

[72] *Vide*, Curve *A*, in Plate XXVIII, *supra*, p. 266.

[73] *Vide*, Plate XVIII, *supra*, p. 239.

He thinks that number-superstitions do not explain number-preferences but that both "spring from a similar psychical condition."

> There must be something peculiar about a number to which superstition or symbolic meaning may cling; it must somehow stand out in consciousness. The emphasizing feature may be something in the numerical relations themselves (as 30 is the sum of the first ten numbers of the series, to use one of Dresslar's instances), or it may be some relation in nature, as man's having five fingers on each hand, or the quarter of the lunar month being seven days, or perhaps some purely accidental relation—but whatever its nature, it must make the number prominent in consciousness before it can become a matter of superstitious regard. Now in such guessing as we have been considering, mere prominence in consciousness, or mere ease of return to consciousness, for any cause, is sufficient to determine a preponderant frequency in the guessing. Superstitious importance when once established may easily contribute to the prominence of a number, and so increase its frequency in the records of the guessing, but its influence is indirect and much modified by other considerations. (p. 399.) . . . All the odd numbers stand out above the even for purely numerical reasons. They present a certain solidity because they are not divisible by two, and among the odd numbers 3 and 7 over-top the rest; for 9 is not prime, 5 is common and easy from its connection with the decimal system, and 1 from its simplicity and complete familiarity. To such original means of emphasis as this is added the repetition and fixation in attention due to superstitious or symbolic conceptions, and all combine to determine the otherwise undetermined digits in the number guessed.[74] (pp. 399-400).

Although the stubborn, refractory nature of the prime numbers is no doubt responsible for the emphasis which they enjoy in the traditional consciousness, to the individual laboring over his arithmetic they become discriminated before tradition becomes operative. And the various digits take upon themselves, in the individual consciousness, varying characteristics which must be largely determinative in establishing mental habit with respect to their use.

Some individuals have chromæsthesia, or other synæsthesias, for names, letters, and numbers,[75] *i. e.*, names, letters, or numbers are seen or heard as colored, and the affective value of the color would determine mental habit with respect to them. With respect to names, Professor Dresslar once asked a subject whether the associated colors influenced her preference, and received the following reply: "They do. I do not like those names associated with the reds. I like bluish names . . ." [76]

[74] Sanford: On the guessing of numbers. *American Journal of Psychology,* 1903, 14:383-401.

[75] *Cf.,* Galton: Enquiries. 1883, p. 149; Titchener: Text-book, p. 195; *American Journal of Psychology,* 11:377; 18:341.

[76] Dresslar: Are Chromæsthesias variable? *American Journal of Psychology,* 1903, 14:380.

But since synæsthesia is probably not more common among people than partial color-blindness, it cannot be a large determinant of number preferences. But, if, as Titchener suspects,[77] synæsthetic experiences are not to be traced back to associations formed in childhood, but are due to some anatomical or functional peculiarity of the individual's nervous system, they are to be noticed as a distinct factor in the determination of mental habits.

The more frequent determining factors are likely to be the associations built up in the individual's experience. To illustrate, the writer still has a very definite scale or system of mental attitudes which the various digits arouse; and these are directly consequent upon the number-experiences of his childhood: *2*, *4*, and *8* are relatives, they are kindly and responsive; they retain some vestiges of attributes with which they were once more definitely endowed; they are republicans, for example, and church-goers, as the intimate friends of his family were. On the other hand, *3*, *6*, and *9* are strangers, somewhat hostile, and troublesome; they are democrats, and they run their harvesters on Sunday; but *6* fraternizes with friends and is to be tolerated although the rôle it plays in the multiplication table is against it. *5* is a lucky acquaintance that is too seldom met, and belongs to the family of the tens and hundreds. *7* is the Hun of the lot; frustrates almost every enterprise that involves it. *1*, also, is individual, is likely, but really doesn't count much. *0* is a quandary; shares a rival claim with *1* for first place in the series; doesn't count in addition but has a deadly effect in multiplication. *4* becomes distinguished; it is $2+2$, 2×2, and 2^2; *9* stands next in dignity as 3^2. *1*, *3*, and *7* have also undergone change owing to associations in addition to those derived from ease of arithmetical manipulation: *1* is the symbol of unity; it is regal, self-contained, final; from it all numbers take their origin, and to it they all, at n^0, return. *3* has been advanced by many triune corporations—the Trinity, the triumvirate, the syllogism; by structural associations—the rigidity of the triangle, and of the three tent-poles; by social facts—the minimum number of branches of a government, or of members of a family. *7* has come up through the plagues of Egypt and the lean years, Jacob's service and Nebuchadnezzar's madness, Rome's topography and the Wonders of the World, not to speak of the wise or the blind men, the deadly sins or the ages of man, or the colors in the spectrum or the notes in the musical scale, a cosmopolitan and fascinating Othello; and he has through conspiracy with *4* and

[77] Titchener: A Text-Book of Psychology, 1911, p. 197.

Juno, effected a perpetual hebdomadal memorial which is published all over Christendom in the calendar. Thus were the digits first personified, and later subjected to various influences; preferences can scarcely be expected to escape the influence of these early associations.

Acquired associations may also determine preferences for letters, through connection with names, or by reason of æsthetic evaluation of form; for cards, through value in play. In any case, the associations are likely to be many, and which one becomes determinative upon any given occasion depends upon a multitude of factors a discussion of which would involve one of the most complex chapters in psychology—on the incentives to, and liability of, reproduction.[78] In many cases, owing to like training, mental habits are similar, and, in guessing, work for coincidence. In general, it has been said, we are mental communists; preoccupied upon the same theme, the ideas which arise in the minds of friends for expression are similar, often identical. It is, of course, owing to the fact that human minds are similarly constituted that a science of psychology is possible. It is owing to the same fact that communication is possible and that language has developed.

Some of the experimental results of the word-reaction test are pertinent, in exemplifying psychical communism. Thumb and Marbe have shown that when a large number of persons make a word-reaction to a given stimulus-word the responses are very often identical.[79] And Reinhold has shown that the agreement in responses of school children to given stimulus-words increases with age or school-grade.[80] Kent and Rosanoff, who sought characteristic differences between the responses of normal persons and the responses of abnormal subjects, gave 100 stimulus-words to 1000 normal persons in order to get normal frequency-tables of the responses to each word.

> The total number of different words elicited in response to any stimulus word is limited, varying from two hundred and eighty words in response to *anger* to seventy-two words in response to *needle*. Furthermore, for the great majority of subjects the limits are still narrower; to take a striking instance, in response to *dark* eight hundred subjects gave one or another of the following seven words: light, night, black, color, room, bright, gloomy; while only two hundred gave reactions other than these words; and only seventy subjects, out of the total number of one thousand, gave reactions which were not given by any other subject. (p. 8 of reprint).

[78] *Vide,* Külpe: Outlines of Psychology, 1901, pp. 196 ff.

[79] Thumb and Marbe: Experimentelle Untersuchungen über die psychologischen Grundlagen der sprachlichen Analogiebildung. Leipzig, 1901, pp. 17 ff.

[80] Ferdinand Reinhold: Beiträge zur Assoziationslehre auf grund von Massenversuchen. *Zeitschrift für Psychologie,* 1910, 54:184.

Their general conclusion was:

> On a general survey of the whole mass of material which forms the basis of the first part of this study, we are led to observe that *the one tendency which appears to be almost universal among normal persons is the tendency to give in response to any stimulus word one or another of a small group of common reactions.* (p. 14).
>
> It appears from the pathological material now on hand that this tendency is greatly weakened in some cases of mental disease. Many patients have given more than 50 per cent of individual reactions.[81] (p. 14).

This psychical communism, then, develops gradually in the experience of the child, is a striking characteristic of normal adults, and is impaired by certain kinds of mental disease. It molds experience in like forms, effects common associations, common scales of evaluation, common preferences; it explains why mental habits are often common. But the psychical factors upon which mental habit rests are not communistic; they are individual: the material of experience and the incentives to, and the liability of, its reproduction. This explains the large individual variation in mental habits. Were reproduction not variable and selective, with respect to the various materials of experience, mental habit would not be shown in such judgments as we have examined. Individual variation in this psychical factor accounts for the individual variation in the extent to which judgment is influenced by mental habit.

It is, perhaps, reasonable, then, to expect the influence of mental habit upon all judgments delivered upon matters which permit a fairly wide range of error (star magnitudes, criminal sentence, age of the American living or of the Latin dead, students' grades, cloudiness at Bremen, etc.) and upon matters which permit a still wider range of error (seeds in an uncut monster squash, or beans in a 5-pint bottle). The typical "personal scale" of the systematic errors enforced by mental habit upon judgment of these matters has been found.

Will the same explanation hold for the *équation décimale*, the "personal scale" found in judgments of the decimal divisions of small temporal and spatial magnitudes? Are the systematic errors in estimating the tenths of a degree on a thermometer, calibrated for each degree only, dependent upon number-preference, upon the unequal liability of the various tenths to come to mind? This sort of influence we have found to exercise the greater control over judgment in situations which permit the wider ranges of error; as in estimating a student's merit in a percentile grade, which, because the least discriminable values lie about 8% apart,

[81] Grace Helen Kent and A. J. Rosanoff: A study of association in insanity. *Am. Jr. of Insanity*, 1910, 67: 37-96, 317-390.

permits a "probable error" of ±4%. The estimate is determined by the greater liability of one of the unit's digits over the neighboring seven digits to come into consciousness. The decimal divisions of a degree on the thermometer-scale, however, will not permit so wide a range of error. And in so far as this range is decreased is the opportunity for number-preference decreased. The range of the "probable error" in the thermometric observations reported from Dundee (for the distribution see Plate XXI, Curve *D*)[82] is only .54 of a decimal part of a degree; and the range on the scale at the calibration upon which the *0* judgments were made is 1.75 of a decimal part. Lewitzky,[83] in applying Zinger's mass correction which he found in high agreement with Gonnessiat's, to the transit-observations of Boquet's observers,[84] learned that for some of the more neglected tenths the systematic error for some of the observers amounted to a full tenth of a second. Obviously, since the range of error is not greater than a decimal part, the "personal scale" cannot be caused by number-preference. A good proof that the large error on *0* is not caused by a preference for a round number lies in the fact that when instruments are calibrated on decimal parts of a degree those decimal parts are as often favored as *0* is. Curves *D* and *E*, Plate XXVI,[85] illustrate this with respect to the half-second standard furnished by clock-strokes; and Curves *J* and *K*, in the same Plate, with respect to the .5° calibration on the thermometer. With the .2° calibration on the thermometer we get the distributions shown in Curves *A*, *B*, *C*, and *D*, Plate XL.[86] Again, when the estimates are made in tenths and half-tenths of spaces on record-sheets (Libellen) the distributions are not essentially different from those of estimations made in tenths only, as may be seen from Curves *E* and *F* which are drawn from data presented by Meissner.[87] The large positive systematic errors fall upon the calibrations, regardless of what decimal divisions are calibrated, and of whether the estimations are made in tenths or half-tenths. The fact is that observers have a "personal scale" which is not imposed upon them by

[82] *Supra*, p. 251.

[83] Astronomische Nachrichten, 1890, 125:75-6.

[84] *Vide*, Curves *A*, *B*, *C*, Plate XXIV, p. 257.

[85] *Supra*, p. 261.

[86] These distributions are reported by Hellmann (*op. cit.*, p. 286) for Wasserleben, Potsdam, Celle, and Pawlowsk. The observations in each case, except for Celle, cover a period of ten years and number 10,956. For Celle, the period covered is 20 years, and the observations number 21,912. At this last station the observations were made by a single observer. At Pawlowsk the observers were said to be the more skilled.

[87] *Op. cit.*, pp. 139-142. Curve *E* represents 7898 observations made by W.; Curve *F*, 6552 made by K.

number-preference, and which is at variance with the objective scale which they endeavor to adopt. For the observers on Pikes Peak,[88] for example, the "personal scale" varies from the objective scale as is shown below:

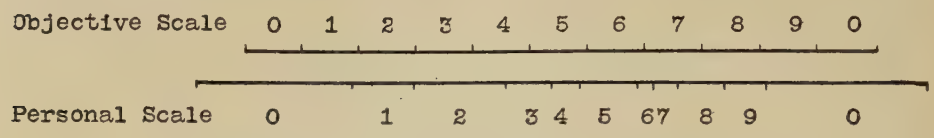

Fig. 4.— A comparison of the objective with the "personal scale" of the meteorological observers on Pikes Peak. The mid-points of the *0* divisions are in apposition.

On the decimal divisions of *1* and *5* only is there no considerable error. On division *6* only a quarter of the expected number of estimates have fallen; on *0*, 2.76 times the expected number. The distribution of

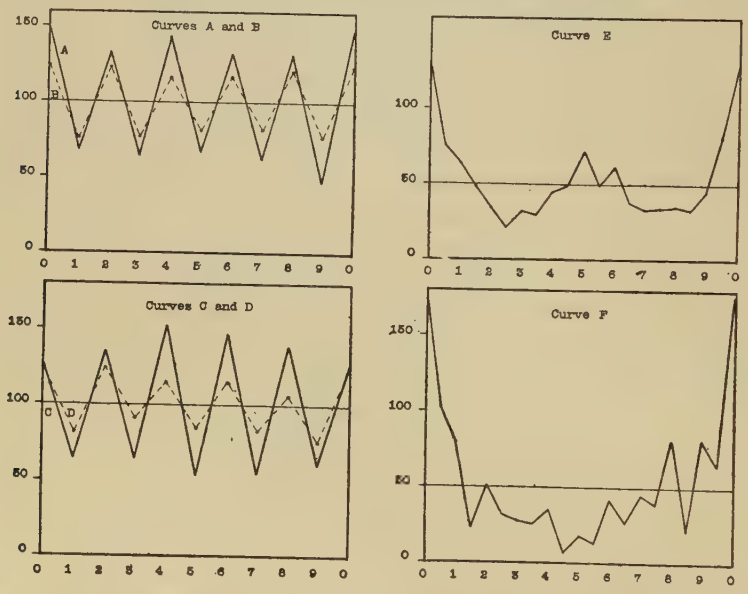

Plate XL.— Curves showing the Dependence of the *'équation décimale'* upon cali-bration-marks, and its independence of fractionation of decimal estimates.

Curve				
A.	.2° Thermometric observations at	Wasserleben.	10,956 cases.	(Hellman.)
B.	" " " "	Potsdam.	10,956 "	"
C.	" " " "	Celle.	21,912 "	"
D.	" " " "	Pawlowsk.	10,956 "	"
E.	(Libellen) Estimates in tenths and half-tenths.	Obs. W.	7898 "	(Meissner.)
F.	(Libellen) Estimates in tenths and half-tenths.	Obs. K.	6552 "	"

[88] *Vide*, Curve *A*, Plate XXI, p. 251.

the observations is just such as would be expected had the observers held the "personal scale" pictured above accurately in mind and employed it without error.

The causes of this mental habit are probably very complex, and are not all psychical. Walter [89] sought to determine whether it varied with the distance between the calibrations, and had 500 estimates made from a recalibrated thermometer, with each of the following distances: 0.8, 1.2, 1.6, 2.0, 2.4, and 2.8 mm. The deviations on the *0* were, per M, 119, 45, 32, 55, 68, 84; and the average deviations: 44.8, 25.7, 26.8, 17.3, 28.3, 28.8. This indicates that the optimal distance between calibrations is 2 mm.; and that the error in the "personal scale" increases with a departure in either direction from this magnitude. He pointed out that instrument-makers should be better informed, since they commonly place calibrations so close together as to preclude reliable reading, sometimes to the extreme of using a third of the space for calibration-marks. The calibration-mark in any case occupies a part of the distance which, in estimating decimal parts, must be considered as unfilled space. The physical fact of coincidence of the top of the mercury with some level of the calibration would, consequently, constitute one cause for increasing the frequency of the *0* estimate. One psychical cause acting in conjunction with this is the well-known tendency to convert "no difference" into "like" judgments;[90] and another is the tendency to underestimate small magnitudes in comparison with large magnitudes, especially when they receive a disproportionate degree of attention. To the last cause Bauch [91] attributed the piling up of estimates upon the terminal tenths which he found in his results. Both of these psychological causes would also operate on the .5 division which can be fairly accurately located. Four further psychical causes may operate to disturb accuracy in the locating of the .5 point: (1) If the scale is vertical, owing to a well-known space illusion, the midpoint is placed too high; the lower spaces of sensibly symmetrical letters, to illustrate, are larger than the upper spaces as may be seen if the letters are inverted— S S, 8 8, (2) If the scale is horizontal, and monocular vision is used as in transit-estimation, the external segment is overestimated; *i. e.*, with the right eye the right half is overestimated and the mid-point is located too far to the right. (3) Whether the scale is vertical or horizontal the half of the space between calibrations which is intersected by the surface of the mercury is

[89] *Op. cit.*, pp. 249 ff.

[90] *Cf.*, F. Angell: On judgments of "like" in discrimination experiments. *American Journal of Psychology*, 1907, 18: 253-260.

[91] *Op. Cit.*, p. 219.

likely to be overestimated, since an unfilled space seems less than a filled or a divided space of the same size.[92] And (4) if the mercury-filled part of the space and the unfilled part differ greatly in shade, as black and white, the black segment is underestimated. Feet or hands appear smaller in black than in white. This last source of error was noted by Walter.[93] If the .5 is mislocated, the other intermediate divisions are disturbed; and the same causes of error apply directly to them also. Other psychological sources of error which would account for individual differences in the "personal scale" refer to characteristic methods of making the estimate, and have been noted by the astronomers. Hartmann [94] said that observers differ in making calculation from the first line, from the second, or from the estimated middle of the space between them. Gonnessiat [95] noted that some observers estimate the *1*, *2*, and *3* decimal divisions absolutely, without comparison with the complementary spaces. And Grossmann [96] contended that proper estimation is not based upon the image in the eye but upon the sensation of the eye-movement over the points of reference; consequently the position of the head is important, since tortion must be avoided. Imperfections of the eye, such as asymmetry of the retina, astigmatism, etc., are still further causes of individual variation. This brief resumé of some of the causes of the "personal scale" may, perhaps, suffice to indicate that the process of estimating decimal parts of small spatial magnitudes is sufficiently complex to permit the operation of many factors of mental habit, without pursuing these factors *ad nauseam*.

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[92] These three causes are due to asymmetry of the eye-muscles. *Cf.*, Wundt: Human and Animal Psychology, 1912, pp. 156 f; or Grundzüge der Physiologischen Psychologie, 5te Auflage, 1902, vol. II, pp. 548 ff.

[93] *Op. cit.*, p. 252.

[94] *Op. cit.*

[95] Gonnessiat: Recherches sur les erreurs personelles dans les observations de passages. *Bulletin Astronomique*, 1889, 6:471-480.

[96] *Op. cit.*, pp. 157 ff. *Cf.*, Wundt: Human and Animal Psychology, 1912, p. 151.

APPLICATION OF MENTAL HABIT TO EXPERIMENTS IN THOUGHT-TRANSFERENCE.

The recognition of the influence of mental habit upon judgment is very important for all those who either conduct experiments in thought-transference or presume to interpret the results of such experiments, for the reason that, should the conditions of experiment permit it, the effect of common mental habits constitutes an extra-chance cause which the quantitative results would not fail to show and which might, consequently, be easily mistaken for thought-transference. The more rigid the mathematical and statistical treatment of the data, the more definitely would the extra-chance influence be revealed and the more certain would the error in interpretation be to bear vicious fruit.

For example, suppose the German reagents whose number-preferences are shown in Plate XXXII, Curve *B*,[97] were acting as experimenters, and our own reagents whose number-preferences are shown in Curve *A* on the same Plate were doing the guessing, in thought-transference experiments in which the guesses were made on the digits from *1* to *10*, and the experimenters chose at random the digits to think of. Since the experimenters think of the intermediate digits much more often than they think of the others, and the reagents guess much more often those same digits, it is obvious that R guesses would be more frequent than chance provides for. This fact is more apparent if we examine an extreme case. Suppose the experimenters had a preference for *5* and thought of it in one-half of the experiments, and the reagents guessed *5* in one-half of the experiments, then the chance of R guesses on all the digits would greatly exceed .1, the normal probability of a single coincidence: half of the guesses on *5* would be right, yielding a ratio of .25 R cases; and ⅑ of the remainder would be right, yielding, say, .05 R cases; aggregating .3 R cases, an excess of .2 or 20%. And this excess, indicating an extra-chance cause, results not from thought-transference but from similarity in mental habit with respect to thinking of digits. Of course, the influence of similar mental habits is not so extreme as here considered, but in so far as it is operative it works for R cases. Before determining the effect of the influence when exerted in the degree shown in the curves referred to above, it may be well to examine a parallel case which is perhaps clearer than a thought-transference experiment.

[97] *Supra*, p. 273.

From it we can derive a formula that will be applicable to the digit-guessing.

Let us illustrate the mathematical situation with the throws of two dice. If they are perfect homogeneous cubes, each face has an equal chance with any other of being thrown, and, since there are six faces, that chance is $\frac{1}{6}$ of the total number of throws. In the throws of die *a*, then, the "probability of occurrence" of the ace is $\frac{1}{6}$; of die *b* also $\frac{1}{6}$. If dice *a* and *b* are thrown together the probability of getting the ace with both is $p \times p$, or $\frac{1}{6} \times \frac{1}{6} = \frac{1}{36} = .0278$. Should the actual number of coincidences obtained in a series of throws exceed this ratio by an amount greater than the deviation allowed by the theory of probability, an extra-chance cause working for the coincidence of the aces would be demonstrated. Suppose the dice are "loaded" so that the probability of occurrence of the ace is greater, say $\frac{1}{5}$ for each die; then, the probability of coincidence would be $\frac{1}{25}$, and in a series of throws of sufficient length an extra-chance cause would be demonstrated.

If we are interested in all of the coincidences, as we are in thought-transference experiments, then the probability of coincidence would be the sum of the chances for the coincidence of the respective six faces; $\frac{1}{36} + \frac{1}{36} + \frac{1}{36} + \frac{1}{36} + \frac{1}{36} + \frac{1}{36} = \frac{6}{36} = \frac{1}{6} = .167$. But if the dice are similarly loaded so that a long series of throws shows, in per cent, the following occurrences of the respective faces

	Face	*1*	*2*	*3*	*4*	*5*	*6*
Die	*a*	10	12	27	6	22	23
"	*b*	12	13	26	7	21	21

then the probability of coincidence is definitely increased: If the respective probabilities of the occurrence of the ace with dice *a* and *b* may be represented by p_1 and p_2, then $p_1 = .10$ and $p_2 = .12$, and the probability of occurrence of a coincidence of aces is $p_1 p_2 = .10 \times .12 = .0120$, instead of .0278; and of the 3-spot $p_1 p_2 = .0702$. A summation of the six $p_1 p_2$ values gives, for all the coincidences, .1965 instead of .167, indicating a theoretical extra-chance value of $.1965 - .167 = .0295$, or about 3%.[98] This is the effect of the loading of the dice.

[98] Generalizing, we may derive a formula for determining the theoretical amount of such an extra-chance cause resulting from loaded dice, or from the influence of mental habit in guessing, and displaying itself in the unequal chances of the single events:

$$x = \Sigma (p_1 p_2) - p,$$

in which p_1 and p_2 are the respective probabilities of the occurrence of a given event (die-spot *1* or *2* or *3*, etc.) in the two series, Σ is the sign of summation, and p is the probability of occurrence of a single event under the condition that all the events have equal chances.

Recurring to the digit-guessing, we find by the use of the formula just derived that the theoretical ratio of R guesses would be .115, the influence of mental habit, consequently, amounting to .115 — .1 = .015 or 1.5%. This illustrates the rôle that a fairly general mental habit may play in thought-transference experiments. But we have learned that individuals vary greatly in their digit-preferences, and, consequently, we might expect in a considerable number of sets of experiments, made under the conditions assumed above, to find the results of some reagents indicating a considerable extra-chance cause just because their strong preferences chanced to agree with those of their experimenters.

For example, we see in the lower part of Plate XXXI [99] that Reagents *9* and *44* show some agreement in their number preferences, and Reagents *38* and *45* also. By the application of our formula we find that for the former pair the amount of influence toward R cases would be .029, or 3%, and for the latter, .02 or 2%, should these pairs work together in experiments that permit the mental habits of both members to operate.

Like preferences for individual cards would in a similar way contribute an extra-chance influence for R cases. For example, Reagents *65* and *67*, in the card-guessing experiments have some strong preferences in common. Had either of them acted as experimenter and the other as reagent, and had the card to be thought of been selected at random by the experimenter, not drawn from a shuffled pack, the amount of extra-chance influence as found by application of our formula would have been .0283 or 2.8%, which is larger than the probability of a single occurrence of an R case (2.5%).

It must be recognized, then, that whenever the experimenter (agent) selects at random one of a definite series of things to think of, instead of drawing it by chance, the unconscious influence of mental habit may result in the expression of preferences which, if in agreement with those of the reagent likewise induced, constitute an extra-chance cause working for R cases.

Statistical treatment may, indeed, remove the cause by such an artificial selection of the data as removes the operation of the agent's preferences;[100] but this can be done by a reader only when, as is not usual,

[99] *Supra*, p. 272.

[100] This could be done by counting the R cases in an *equal* number of guesses on *each* of the numbers or cards, care being taken to take every guess on each number or card from the beginning of the series until the quota is obtained; or, by dividing the number of R cases on each card guessed by the number of guesses made on it, reducing to a common denominator, and adding.

the original series of data are published. Obviously the admission of the influence of mental habit into thought-transference experiments is a serious error.

But, it is worth noting, while we have a statistical measure of this extra-chance influence before us, this error is not so great as some critics intimate. In order for the above deviation of + 2.8% in R cases on the Card, to be certainly established as an extra-chance cause, the number of experiments upon which the per cent is based must be about 600.[101] And in order for the 3%, found above as a measure of the influence of mental habit in digit-guessing, to be satisfactorily demonstrated the number of experiments would have to be 2000.[102] These figures serve to show that even when we select from our data the cases of greatest similarity between the mental habits that have influenced the guesses to the greatest degree, the extra-chance influence is not really large. As an explanation for a large deviation from probability in the results of a long series of thought-transference experiments—such, for example, as the Creery experiments (see Appendix C)—it is unsatisfactory.

For a smaller deviation above probability, however, this explanation may be legitimate, as has proven the case in an interesting investigation conducted by Professor E. C. Pickering[103] of the Harvard College Observatory during the early days of the old American Society for Psychical Research. He observed that if thought-transference is, as Richet suspected, commonly operative to a substantial degree among normal persons, an important error might be entering the observations of the star-magnitudes with which the Observatory was just then engaged, in conjunction with other observatories, in the revision of Argelander's Durchmusterung. The observer, after familiarizing himself with the Durchmusterung scale of brightnesses, estimated the brightness of each star observed. After each estimate, the Durchmusterung magnitude was read aloud by the recorder, to enable the observer to continually correct his scale. Since the Duchmusterung magnitude was commonly in the recorder's mind while the observer was forming his estimate, there was the opportunity for the estimate to be supplied by thought-transference from the mind of the recorder. If this process occurred, there would be an excess of zero deviations of the estimates from the Durchmusterung magnitudes. A large number of observations—about 50,000—of the stars between +50° and +55° were at hand for inspection. Of these,

[101] *Vide, supra,* Table XLII, p. 90.

[102] *Vide, supra,* Table XLI, p. 89.

[103] Pickering: Possibility of errors in scientific researches, due to thought-transference. *Proceedings Am. S. P. R.,* 1885, Series I, 1:35-43.

7568 observations in 0, 6, 12, and 18 hours of right ascension were deemed sufficient for statistical treatment. There were three observers (*R.*, 3128; *P.*, 3248; *W.*, 1192) and five recorders, which permitted an analysis of data for testing the results of various pairs of workers. The deviations of the observations from the Durchmusterung magnitudes were tabulated according to size and their distribution was compared with the theoretical distribution. There was no significant deviation in the distribution of Observer *P.*; but in the distributions of Observers *R.* and *W.* the excess of *0* deviations was conspicuous, as may be seen in Curves *A* and *B* of Plate XLI. (The abscissæ give the deviations from

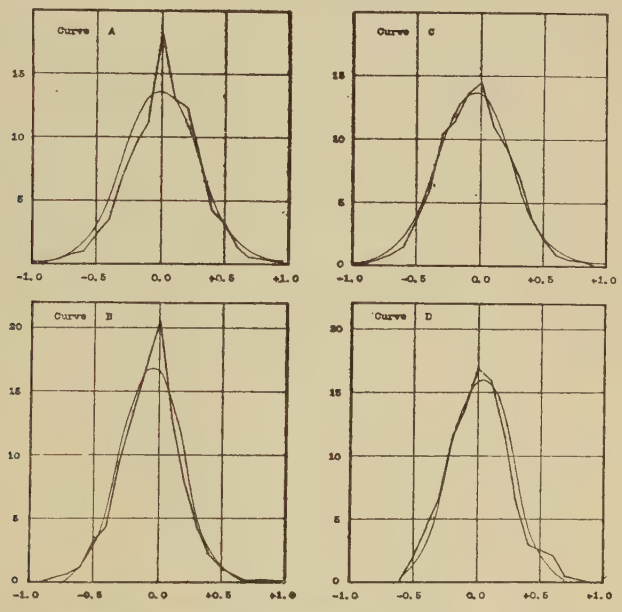

Plate XLI.— Frequency curves of deviations of estimates of star magnitudes from the Durchmusterung magnitudes, showing influence of similar mental habits in excess of coincidence of estimates on *0* deviation.

Curve *A*. Deviations of Observer R.
" *B*. " " " W.
" *C*. " " " R. Mental habit eliminated.
" *D*. " " " W. " " " "

−1.0 to +1.0; the ordinates give, in per cent, the frequency of occurrence. The smooth curves represent approximate probability.) This would be excellent evidence for thought-transference, were there no other causes for this departure from theoretical probability. But, as may be seen from Plates XX and XX*a*,[104] the Durchmusterung is influenced by a

[104] *Supra*, pp. 245 and 246 respectively.

definite "personal scale" which increased the frequency of the estimates upon the *0* and *5* decimal magnitudes, and decreased frequency upon the *1*, *4*, *6*, and *9* tenths, and tabulation of the estimates made by Observers *R.* and *W.* revealed the same "personal scale." Removal of the effect of the common mental habits, by retabulating *an equal number* of deviations of estimates from *each* of the Durchmusterung magnitudes of 8.3 to 9.2, reduced the frequency of the *0* deviation in the distributions of both observers to an amount that falls within the range of chance deviation (see Curves *C* and *D*). Thus a small but significant deviation that at first appeared to be good evidence for thought-transference turned out to be the effect of a common *équation décimale.*

Experiments in thought-transference in which such materials as digits, playing-cards, and estimates of star-magnitudes are used may, in this way, be vitiated to an appreciable degree by the operation of common number-preferences, common card-preferences, and a common "personal scale." But often other materials, not so amenable to statistical treatment, are used in thought-transference experiments for the purpose of getting results the more striking as coincidence is conceived to be the more unlikely. Reliance is placed upon an infinitesimal probability. And here, it may be, mental habit enters with more power to distort the results because of the fundamental and far-reaching similarity of mental processes—because "we are in mental matters all pure communists." [105] Such materials are of the nature of diagrams or drawings, articles of household or personal use, names of places, persons, or literary or historical characters, dramatic events in history or fiction, etc.

To exclude the error introduced by mental habit it is not sufficient that the number of things to be thought of by the agent and to be guessed by the percipient is definitely determined beforehand. Selection of the individual members of the series during experimentation must be effected in a mechanical way—must be drawn by lot, not merely "chosen at random" by voluntary selection. Marbe [106] has shown that when groups of two or three playing-cards are presented to a number of persons, selections have a strong tendency to agree by reason of the influence of common preferences, indicating that the preferences operate when one makes a visual selection as well as when he lets a card come into his mind.

Now, the only series of experiments reported by the American Committee on Thought-Transference that seems to present fair proof of

[105] Minot: *Proceedings Am. S. P. R.* Series I, 1:314.

[106] Karl Marbe: Ueber das Gedankenlesen und die Gleichförmigkeit des psychischen Geschehens. *Zeitschrift für Psychologie,* 1910, 56:241-251.

thought-transference was conducted by W. H. Pickering,[107] of Boston, who was a member of the Council of the American Society for Psychical Research. Of this series the Committee said:

> It will be seen that the agent, Mr. William H. Pickering, considers that by these experiments the reality of thought-transference has been proved as completely as is possible by a single pair of observers. The Committee regret extremely that it has been found impossible to repeat these experiments under conditions which would justify them in expressing an opinion based upon personal observation of the phenomena.[108]

And in the same report:

> It will be seen from this report, that some cases have been brought to the notice of the Committee which seem to indicate that, under certain circumstances, the transference of a conception of geometric form from one mind to another may take place without the use of the ordinary channels of sensation. But these cases are at present merely suggestions for further inquiry. . . . (pp. 114-5).

The material used, which seemed to offer the best conditions for thought-transference, consisted of a series of ten geometric diagrams—a cross, a triangle, a heart, a flag, an anchor, a bell, a star, a circle, a square, a large S, (illustrated in the *Proceedings Am. S. P. R.*, Series I, 1:47). In description of his method the agent said:

> I had the ten figures before me, drawn on a sheet of paper, and selected them at random, taking care to have no method in my selection, such as taking alternate ones or employing other artificial systems. (*Op. cit.*, pp. 114-5).

This is precisely the experimental condition that would permit the operation of preferences, and should the preferences be strong and identical in the minds of both agent and percipient the excess of R cases (19%) due to other causes might easily be brought within the range of chance deviation (9.5%) for the number of experiments made (180). Since the published data will not permit a statistical correction for mental habit, the suspension of judgment in the Committee's report is seen to be justified.

But the agent in his report (*op. cit.*, p. 115) voiced the opinion of some others engaged in psychical research when he said that he prefers "free drawings" as material for experiment. Reliance would be placed upon an infinitesimal probability. Here, however, mental habit may operate to such a degree as to raise the probability of R cases to a very substantial figure,—and the situation is calculated to baffle all statistical

[107] Wm. H. Pickering: Experiments on thought-transference. *Proceedings Am. S. P. R.*, 1886, Series I, 1:113-6.

[108] *Ibid.*, p. 110.

calculation, fruitful of controversy without end among those who seek to interpret the results.

In order to determine the approximate degree to which mental habit operates in thought-transference experiments in which "free drawings" are used, Professor Minot[109] sent out to members and friends of the American Society for Psychical Research a large number of blank postal cards, with the printed request: *"Please draw ten diagrams on this card, without receiving any suggestion from any other person, and add your name and address."*

He received for statistical analysis 501 cards, 310 from men, 169 from women, and 22 unsigned. His first table shows that

> There is an enormous preponderance of a few figures, a great preponderance of some others, and a certain preponderance of still others. (p. 304).

The order of preference was: Circles 287, squares 236, triangles 220; four-sided figures 245, other straight-sided figures 149, "making of these very simple figures 1137, or over one-fifth of the total number" (p. 305). There was less variety among the diagrams of the women than among those of the men, showing a greater influence of mental habit which would play a heavier rôle were the percipient a woman, and a still heavier one were both agent and percipient women.

> [Beyond the diagrams resulting from specially personal preferences, and occupational preferences, there were "a considerable number" obviously] suggested by the objects around the persons when they were making the diagrams, or some association of ideas, or by the recollection of objects or figures with which they had been specially or even only casually occupied shortly before; but the great majority are of such a character that we need not hesitate to designate them as thrown out of the mind, or as *ejective* . . . (p. 313). The images and notions which pass across the consciousness of each individual are almost all common property. . . . Our thoughts are in large measure owned by the community; we are in mental matters all pure communists. . . .
>
> It is evident that if two persons are requested to think of some one thing of a class, such as a letter of the alphabet, a playing-card, a baptismal name, there is by no means an equal chance of their selecting any one; on the contrary, there is not only the probability that they will think of a special one first, but there is a chance of their both thinking of the same one, for the relative frequency or preponderance of one idea or image out of a set has been shown to be similar for a number of people. In order to prove the reality of thought-transference, it must be demonstrated that the observed coincidence of thoughts can *not* be explained by the law of relative frequency.
>
> Let us suppose by way of illustration that two persons make an experiment in thought-transference with diagrams. The agent draws a circle; now, four per-

[109] Minot: Second report on experimental psychology:—upon the diagram-tests. *Proceedings Am. S. P. R.*, 1889, Series I, 1:302-317.

sons out of ten are likely to draw a circle, and to draw it near the beginning of a series of diagrams; instead, therefore, of the chances of the percipient's drawing a circle being almost infinitely small, they are very great. The trial is proceeded with; the circle having been drawn, it is probable that the next figure will be different, as our cards show; the agent draws a square; again the percipient's unconscious chances are very great. And so on with a considerable series of diagrams. In this manner thought-transference might be simulated, and a proof of its reality obtained, which would seem overwhelming so long as the law of relative frequency is disregarded as an explanation. (pp. 314-5).

Professor Minot in the light of his results examined several series of experiments published by the English Society, including the "free drawing" experiments of Herr Schmoll [110] concerning which that investigator said:

The results of the preceding trials clearly leave much to be desired; nevertheless, it is not to be denied that in many cases the reproduction possesses the fundamental character of the original, and, indeed, in many (as, for example, Nos. 2, 8, 12, 13, 18, 21, 24, 25) very strongly approaches precision. In no single case, strictly speaking, did there appear absolute discrepancy between the form of the reproduction and that of the original. We have therefore been able to convince ourselves that the agents, concentrating their looks on the given object, projected on the mental eye of the percipient a picture more or less resembling it, and we take it as incontrovertible that the above results could not have been achieved by conscious or unconscious guessing. (p. 336).

And he (Minot) said:

If we examine the drawings . . . we notice at once that . . . the figures drawn by both the agents and percipients are in greater part just such as our diagram-tests have shown to be the ones likely to be drawn. The authors of the articles in question having fundamentally misconceived the nature of the chances, of course fail to offer the necessary proof that the proportion of coincidences was greater than chance would account for. (p. 315).

He even went further, saying

If Messrs. Blackburn and Smith had observed that there are, say fifty diagrams which people are likely to draw, a code could have been easily arranged for the former to signal to the latter which one or two of the diagrams had been drawn. If, further, the code include signals for straight lines, for semicircular curves, for right, left, up and down, or below and above, it would not be very difficult nor require long for a couple of expert collusionists to accomplish the thought-transference of almost any of the diagrams in the series given in the pages cited. I do not bring any accusation against the two gentlemen who achieved the remarkable successes reported by the English committee; I merely point out that the hypothesis of fraud still remains tenable, and that unless it is met adequately,

[110] Anton Schmoll: Experiments in thought-transference. *Proceedings S. P. R.*, 1887, 4: 324-336.

persons of cautious judgment must consider that the explanation of the success of Mr. Smith in the reproduction of drawings is more probably fraud than supersensuous thought-transference.

If this view is adopted, the general conclusion is unavoidable that none of the experiments heretofore published afford conclusive evidence of thought-transference.[111] (p. 316-7).

To this sweeping conclusion there were, of course, vigorous protests, the justice of which will be presently shown. But Minot made his point. He showed an important source of error based upon "the relative frequency of ideas," and properly charged the English investigators with failure upon some occasions to take account of it.

An investigation was soon after made by Lieut.-Colonel G. Le M. Taylor [112] to test Minot's "law of relative frequency." He "prepared 40 sheets of paper by marking off on each side 25 square spaces headed 'Please draw 25 diagrams without receiving suggestions from any person, one in each of the spaces below, running down each column in succession, beginning at the top of No. 1.'" He numbered the columns from left to right, and marked the sheets 1A, 1P, 2A, 2P, etc., so that after they had been filled out by forty friends they would fall into pairs [as 1A (agent), 1P (percipient)] and could be examined for coincidences of drawings. Altogether there were 2000 diagrams, which would correspond to 20 experiments in thought-transference of 50 trials each. Upon comparing, in pairs, his "agent" papers with his "percipient" papers, he discovered only one absolute correspondence (a square), 10 cases of correspondence in idea, and 9 cases of correspondence in shape. There were besides "about 40 pairs having some features in common, but which are not similar enough to be counted." He allowed 20 successes for the 1000 trials.

He then turned to two series of results of thought-transference experiments published in the English *Proceedings* for comparison of the frequency of coincidence of drawings, and found 13 correspondences in 42 attempts. Unfortunately, the series he selected were performed by Blackburn and Smith, the coincidences in which Blackburn later claimed to have been produced by the arts of collusion (see Appendix C).[113]

His reagents, like those of Minot's, showed "a tendency to draw certain diagrams" but not in the same order of frequency; they had a

[111] *Cf.*, Appendix C.

[112] Taylor: Experimental comparison between chance and thought-transference in correspondence of diagrams. *Proceedings S. P. R.*, 1889, 6:398-405.

[113] He included the famous Fig. 22, which we have reproduced in Appendix C.

preference for men, animals, and flowers rather than for simple geometric forms; they too had a partiality for words and letters; their diagrams, however, were more complex in character, more varied, less subject to common mental habits. The differences in the results seem to follow naturally from the differences in the conditions of experiment. Taylor's 40 friends, although they belonged to a single stratum of society and might therefore be expected to express a large number of common preferences, belonged to that stratum of society that, perhaps more than any other since classical times, cherishes intellectual independence combined with charm and grace in personal expression. His request would arouse a psychical liveliness and piquancy foreign to the 501 New Englanders who contributed Minot's results. The drawings which Taylor published give indication of artistic talent and training which one would not expect to find in a sample of 40 persons among Minot's reagents. Statistically, also, one would expect to find greater relative variation in the results of 40 persons, than of 501 persons; and in diagrams drawn to the number of 25 by each reagent than to the number of 10. The experimental disabilities of Taylor's investigation for testing Minot's "law of relative frequency," prevent it from casting any discredit whatever upon Minot's chief contribution.

With respect to Minot's sweeping conclusion affecting *all* published thought-transference experiments, there is room for difference of opinion. If one interprets his strictures as applying the error of mental habit only to those experiments in which the conditions of experiment would permit it to enter,[114] and other experimental errors equally serious to the rest of the experiments, he doubtless has to this day 99.9% of the scientific men of the world with him.

There is, perhaps, not a single series of thought-transference experiments in which "free drawings" were used, or for that matter any other "free" ideas, which can withstand in a respectable fashion the deadly fire of the "mental habit" criticism. A few series do, indeed, show so many correspondences that "mental habit" cannot be an adequate explanation. But they strongly suggest, by the sort of correspondence, that they are due to collusion and fraud. Indeed, for some of them, confessions (in some quarters still discredited) claim as much.

One of the series upon which great reliance has been placed for proof of telepathy-at-a-distance does not escape:

[114] That this was his own meaning may be seen from an "Open Letter Concerning Telepathy," which was a rejoinder to Dr. Hodgson, one of his critics (*vide, Proceedings Am. S. P. R.*, Series I, 1:547 f.).

I am satisfied that in the great majority of cases the coincidences of thought and expression are sufficiently explained by the natural association of ideas in minds preoccupied with the same themes. As an illustration of this I think the experiments of Miss Miles and Miss Ramsden, on which Sir W. F. Barrett lays so much stress, are of some value; whereas their worth as scientific evidence for telepathy-at-a-distance is almost *nil*.[115]

One of the protests to Minot's sweeping conclusions was made by Professor James:[116]

His painstaking study of the diagrams sent in by our associates has given a more definite numerical form to the already well-known fact that simple geometrical figures, letters, faces, houses, and scrawls are the most likely things both to be drawn and guessed in thought-transference experiments where improvised drawings are used. But he seems to me greatly to exaggerate the importance of this diagram-habit when he considers that the absence of special provisions against it in the English Society's experiments constitutes a very formidable objection to their value as proofs of thought-transference.

Our readers will not have forgotten that only a small number of the experiments recorded in the English Society's *Proceedings* were made with diagrams at all. Where diagrams were used, it is true that their *elements* were almost always the familiar ones above mentioned. With so few elements a code of signals is much less difficult than with more; and Dr. Minot consequently infers that where whole series of diagrams were rightly guessed, this may well have been because the agent secretly conveyed information to the percipient by such a code.

He grants the applicability of Minot's criticism to experiments in which "free diagrams" were used except those in which there was "wholesale right guessing," for which Minot had suggested code as a reasonable explanation. Of five specified series he selects as the most striking the two in which Blackburn and Smith were engaged, scouts code, and concludes:

I cannot agree, therefore, that the revelation of the diagram-habit has appreciably weakened the evidence for thought-transference actually to be found in the English Society's reports. To most of that evidence the existence of such a habit is wholly irrelevant; and where it is pertinent, fraud based on its use seems so unlikely, *if the reports are faithful,* that vague suspicions of unfaithful reporting and bad observation seem to me to carry more real skeptical weight with them than Dr. Minot's more definitely formulated charge. (p. 319).

The definitely formulated charge of fraud has, in the light of later events, reported in Appendix C, become more plausible.

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[115] Tuckett: Psychical researchers and "the will to believe." *Bedrock*, 1912, 1:201.

[116] James: Note to the foregoing report. *Proceedings Am. S. P. R.*, 1889, Series I, 1:317-9.

Another protest was made by Hodgson:[117]

I have no desire to underestimate the importance of the considerations depending upon this [number-habit], though the extent of their application is very small as regards the experiments reported by the English Society, being limited to a few of the earliest ones. Professor Minot's research has shown how important it is, in experiments of this kind, to beware that our conclusions are not vitiated by ignoring the possible existence of certain habits in guessing, whatever be the class of objects chosen for experiment. And it may be owing to this research that some later experiments with numbers, recorded not in the *Proceedings*, but in "Phantasms of the Living" (Vol. I, p. 34; Vol. II, p. 653), are not open to the criticism depending on the existence of the number-habit. These experiments appear to have escaped the notice of both Professor Hall and Professor Minot. (p. 532).

He quoted from the latter reference:

The ninety numbers which contained two digits were inscribed on ninety slips of paper, and placed in a bowl. Miss M. Wingfield, sitting six feet behind the percipient, drew a slip at random and fixed her attention on the number which it bore.

Showing that the number-habit of the agent was excluded and that, consequently, the influence of the similar preferences of agent and percipient could not have been operative. He pointed out that in the experiments with playing-cards performed with the Creery sisters [118] and reported by the English Committee, the cards were "drawn at random from a full pack," likewise excluding the operation of the agent's preferences. He also referred to the unfortunate Blackburn-Smith series, as transcending in a crucial way the influence of mental habit in "free diagrams."

Dr. Hodgson protested that the error of mental habit can be properly charged against only a fragment of the English experiments. This is true, but it applies with most force to the "free diagrams" and the free ideas, for which investigators of both societies seem to acquire a strong predilection after they have wrestled with the meagre results contributed by definite materials which permit accurate statistical calculation.

As to the precautions taken by the English investigators against "mental habit," there is evidence, in their provision for drawing the material to think of by lot, that they became aware of the danger near the beginning of their work. As early as 1884 Gurney, in a review of Richet's experiments with the divining-rod said:

It may be worth while to remark that either the selection of the particular hiding-place ought to be settled each time by lot, or the percipient ought to be pre-

[117] Hodgson: On some objections to the theory of telepathy. *Idem*, pp. 528-546.

[118] For an evaluation of these series, see Appendix C.

vented from knowing whether or not his divination has been correct. Otherwise the chances of success may be really affected in the way which M. Richet imagined in the case of the card-guessing. If we allow the *mind* of the agent to govern the selection, then a process in his mind may find its counterpart in the mind of the percipient.[118a]

Mrs. Sidgwick noticed the number-habits of her percipient *T.*, and, after describing them, said, in a footnote at the end of her report:

> It is scarcely necessary to remind our readers that a number-habit affecting the percipient only can have no tendency to increase the number of successful guesses.[119]

In the same number of the *Proceedings,* Myers, in a review of Max Dessoir's "Das Doppel-Ich," discussed quite fully general and idiosyncratic mental habits in their relation to thought-transference experiments, and, in part, said:

> There is *no* choice, I say, however simple or arbitrary—not even the choice between heads and tails or odd and even—which the human mind can be trusted to make as impartially as the spun penny or the roulette-ball would make it.
>
> There will presumably therefore be idiosyncratic number-habits, as well as general number-habits, and although these are not likely to become strong without being observed, still less to become so potent as to explain coincidences in *double-numbers* thought of by two separate minds, it is undoubtedly proper to eliminate this possible source of error from experiments in thought-transference. We have made it a rule, since our first few experiments, to replace numbers in a bag, or cards in the pack, and shuffle between each trial, and draw at random. (pp. 209-210).

And the subject has since been given special attention in several places.[120] It is generally agreed that the influence of mental habit operates to vitiate the evidence for thought-transference only when the agent selects the material to be guessed and at the same time his mental habits are similar to those of the percipient; that in all other cases it would operate to diminish the influence of any other cause besides chance; and that its effect is eliminated by preventing the agent from expressing his preferences, *i. e.,* by drawing the number or card to be thought of by lot.

Although most of the discussion gave almost exclusive consideration to the case of the influence of mental habit upon the guessing of definite materials, such as the ten digits, the two-place numbers, or playing-cards, all the possible occurrences of which are known to the percipient, some attention has been given to the case of the influence of mental habit upon the guessing of indefinite materials, such as "free diagrams."

[118a] Gurney: M. Richet's recent researches in thought-transference. *Proceedings S. P. R.,* 1884, 2:245.

[119] *Proceedings S. P. R.,* 1889, 6:170.

[120] *Journal S. P. R.,* 1899, 9:118-9.

Podmore,[121] under the caption "Special Grounds of Caution," discussing "Thought-forms," in part, said:

There remains one other source of error to be guarded against. An image—whether of an object, diagram, or name—which is *chosen* by the agent may be correctly described by the percipient simply because their minds are set to move in the same direction. It must be remembered that, however unexpected and spontaneous they may appear, ideas do not come by chance, but have their origin mostly in the previous experience of the thinker. Persons living constantly in the same physical and intellectual environment are apt to present a close similarity in their ideas. It would not even be *prima facie* evidence of thought-transference, for instance, if husband and wife, asked to think of a town or of an acquaintance, should select the same name. And investigation has shown that our thoughts move in grooves which are determined for us by causes more deep-seated and more general than the accident of particular circumstances. Thus it is found that individuals will show a preference for certain figures or certain numbers over others; and that the preference for some geometrical figures tends to be tolerably constant . . . (p. 15). . . . If a diagram [is thought of], it is preferable that it should be taken at random from a set of previously-prepared drawings. (p. 17).

And a few years later Miss Alice Johnson[122] said:

In the early days of the Society, some of the most striking results obtained were in experiments in which the percipient attempted to reproduce drawings or diagrams made by the agent. . . . (p. 161).

They were criticised on the ground of the familiar fact that the minds of men have a tendency to run in certain grooves,—so that, for instance, if one is asked to think of or to draw objects, or to think of playing-cards or numbers, each person, though he may not be aware of it, has favourites and is more likely to think of some objects, cards, or numbers, than others. These mental "habits" as they are sometimes called, may be alike in several persons; and when this is so in the case of two experimenters, a certain proportion of the diagrams drawn by the percipient may resemble those drawn by the agent, and thus simulate the phenomenon of thought-transference. (p. 162).

In experiments with drawings, where the number of possible drawings is unrestricted, it is, of course, impossible to calculate how many successes might be obtained by chance; the question can only be tested empirically, and a very large number of trials is necessary to ensure a completely satisfactory test. (pp. 162-3).

The unsatisfactory status of the experimental evidence for thought-transference that is based upon coincidences of "free diagrams" or other "free" material in an indefinite series, because of the influence of common mental habits, suggests another sort of evidence adduced for the proof of thought-transference which is still more unsatisfactory, because it is vitiated not only by mental habits of various forms but also by all those factors that work for the fallibility of human testimony.

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[121] Podmore: Apparitions and Thought-Transference. London, 1894, pp. 15-17.

[122] Alice Johnson: Coincidences. *Proceedings S. P. R.*, 1889, 14: 158-321.

Telepathic dreams, hallucinations, and impressions—so-called "spontaneous cases"—depend, for their record, upon the memory of persons who cannot be presumed to have exercised the ordinary care in observation, in recall, or in report, which is demanded of the commonest scientific observer. These latter disabilities were sufficiently real to cause naturalists to discount the traveler's tale of "a beast with the tail of a beaver and the bill and webbed feet of a duck,"—the ornithorhynchus—just as they still discredit the reports of great sea serpents and flying horses.

In all these cases, so long as the alleged facts rest solely on the testimony of men untrained in habits of close observation and accurate reporting, a suspension of judgment seems to be justified. And if these considerations are valid in ordinary cases, a much higher degree of caution may be reasonably demanded of investigators who leave the neutral ground of the physical sciences to enter upon a field in which the emotions and sympathies are most keenly engaged, and in which the incidents narrated may have served to afford support to the dearest hopes and sanction to the deepest convictions of the narrator. So insidious, in such a case, is the work of the imagination, so untrustworthy is the memory, so various are the sources of error in human testimony, that it may be doubted whether we should be justified in attaching weight to the phenomena of telepathic hallucination and clairvoyance, to which a large part of this book is devoted, if the alleged observations were incapable of experimental verification.[123]

The statistical advantage of the experimental over the spontaneous cases has been pointed out by Professor Richet:[123a]

I do not think that experiments with diagrams have the same demonstrative force as experiments with cards, where the chances are exactly known.

And also by Miss Johnson:[124]

. . . In successful experiments dealing with the events in a chance series (*e. g.*, experiments in guessing cards or numbers) it is not necessary to allow anything for the action of inference. For this reason, such experiments afford more satisfactory proof of supernormal power than spontaneous cases. Incidentally, they possess the further advantage that the degree of probability of success in them is not a matter that admits of difference of opinion. . . . (p. 183).

. . . In considering events that are causally connected, there always are, from the nature of the case, rational grounds for inferring from one event something about another one, and inference is then likely to lead us right oftener than wrong on the whole. (p. 183).

The causal relations between the events, that for the purpose of statistical treatment should be independent, may lie in associations which the persons concerned share in common; and since there are associations

[123] Podmore: Apparitions and Thought-Transference, p. 5.

[123a] *Proceedings S. P. R.*, 1889, 6:69, footnote.

[124] Alice Johnson: Coincidences. *Proceedings S. P. R.*, 1898, 14:158-321.

of all degrees of commonness the range of error due to inference, so far as persons are concerned, is unrestricted. This sort of error depends more upon the psychical connections between ideas which determine their sequence than upon the frequency with which ideas arise in the mind. It is consequent upon the facts that human experience is integrated and that it is communistic. The word-reaction experiment provides a typical illustration of both the extent and the force of this influence. As was shown some pages above,[125] the responses of 1000 normal persons to the stimulus-word *needle* were all comprised in 72 words, and the responses of 800 persons to the stimulus-word *dark* were all comprised in 7 words. A modification of the word-reaction toward the form of a "chain of ideas" illustrates the manner in which the suggestive force of many common associations may be converged into determining power. Last semester the writer by way of demonstration conducted a class-exercise in which he pronounced three links of a chain of ideas, each member of the class being required to record immediately three further ideas. It had been explained with the assistance of an illustration that the ideas recorded should be the first successive three that came to mind after the writer's third was given. The stimulus-ideas were, *Lafayette, Delaware, cherry tree.*

The number of students who responded was 160.

For the 4th idea, 93 gave *Washington*, 18 gave *hatchet*,
" " 5th " 33 " *hatchet*, 13 " *Washington*,
" " 6th " 17 " *lie*, 13 " *Father*.

TABLE LXXIX.

No. of Idea	No. of Responses Comprised in 2 words	in 10 words	No. of Words used in all responses	No. of Students who responded with Hist. associations
4th	111	135	35	149
5th	46	90	68	133
6th	30	66	89	128

Now, it must have been from five to fifteen years since these students learned the legend of the cherry tree and the hatchet, and most of them may not have recalled it for several years, but their responses were quite definitely controlled by the force of its associations, 93 responses for the 4th place being comprised in a single word, 111 in 2 words, 135 in 10 words, and all in 35 words, and 149 of the students responding with historical associations. And, as Table LXXIX above shows, the determining force of the associations continues, though in diminishing degree, in the successive responses.

[125] *Supra*, p. 285.

In the use of "free" materials of indefinite number, in thought-transference experiments, and in "spontaneous cases" of telepathy, coincidences, instead of being infinitely improbable as the infinite number of possible alternatives would seem to indicate, must, because of the suggestive force of common environment and common associations, be highly probable.

The case for thought-transference will have to rest upon experimental evidence derived from the use of definite materials drawn by the agent by lot. In our own experiments this requirement has always been fulfilled.

APPLICATION OF MENTAL HABIT TO OUR EXPERIMENTS ON SUBLIMINAL IMPRESSION.

Since our determination of the influence of subliminal sensory impression upon judgment rested upon a statistical evaluation of R cases in the guessing of letters and digits, it is of interest to learn whether any serious error has been admitted by reason of mental habit, which we now know constantly influences judgment.

In the experiments on Subliminal Impression,[126] it will be recalled, letters or digits were presented by means of a tachistoscope under such conditions as to make the character just not perceptible. The characters were presented in an order determined by chance (slightly modified so that the characters were presented equally often), and had all of the characters (letters or digits) been presented, there would have been no opportunity for mental habit to contribute to R cases. In the first series of the first two divisions, however, only part of the characters were presented, and should the mental habits of the reagents have expressed themselves in preferences for the characters which were chosen for presentation, just to that extent would they have contributed to R cases.

1. Let us examine first the 1527 guesses of letters and digits made in the experiments of 1915-16 and tabulated in Table LXII.[127] The characters presented were *B, H, K, U, Z, 2, 4, 5, 7, 9*; but the reagents were not told that there was any restriction of the alphabetic or numerical series, were asked to guess any letter or any digit—the first one that came to their minds,—and were expected to distribute their guesses impartially over the combined series. The probability of R guesses is $\frac{1}{36}$ or 2.8%.

The following is a distribution of their guesses:

A	*B*	*C*	*D*	*E*	*F*	*G*	*H*	*I*	*J*	*K*	*L*	*M*	*N*	*O*	*P*	*Q*	*R*
24	113	51	23	17	17	54	72	11	9	53	50	59	26	55	61	12	57

S	*T*	*U*	*V*	*W*	*X*	*Y*	*Z*	*0*	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*
38	25	30	4	11	10	5	132	0	7	39	46	71	62	66	72	84	61

Although individual reagents had strong preferences for individual characters, as, *A, B, G, H, M, R, Z, 3,* and *8,* and curiously limited their guesses often to less than half of the combined series, they showed com-

[126] *Vide, supra,* pp. 190 ff.

[127] *Supra,* p. 193.

mon preferences in their aggregate results for *B, H, Z, 4, 6, 7,* and *8,*—most of the characters chosen for presentation. Of course, some of the excess of guesses upon these characters is due to the direct influence of subliminal impressions resulting in R cases, and some of it, as has been suggested,[128] may be due to a mental disposition effected by the subliminal impressions which expresses itself in increasing the frequency of guesses on the exhibited characters; but both of these effects would be indistinguishable from the effect of mental habit, and for statistical purposes we may assume here that it is wholly the effect of mental habit.

If we aggregate the guesses of the presented characters we get 705 or 46.2%. The most probable per cent is 27.8, which leaves an excess of 18.4%, owing, as we assume, to the influence of mental habit.[129] Since there are 10 presented characters, this excess should contribute 1.8% R cases. The R cases, as shown in Table LXII, were 9.8%. The remaining 8%, less the 2.8% due to chance,(5.2%), is still 2.9 times the limit of chance deviation, and represents the effect of subliminal impression or any other extra-chance causes that could have been operative. No serious error has entered this series by reason of mental habit; we are not sure that it has contributed to R cases at all, but it may have contributed as much as 1.2% allowing for the other extra-chance causes.

2. The guessing of digits presented to peripheral vision also permitted the entrance of mental habit as an extra-chance cause of R cases. A letter and a digit were presented simultaneously—the letter in foveal vision, the digit in peripheral vision—and after the reagent recorded the letter from direct perception he guessed a digit. The digits *2, 4, 5, 7,* and *9* only were presented. In the experiments of Section I the reagent did not know that any digits were being presented and was expectd to distribute his guesses impartially over the digit-series from *0* to *9*. The probability of an R guess by chance is 10%. The results were tabulated in Table LXVIII.[130]

As was noticed in the discussion of the results,[131] mental habits in number-guessing might have contributed to R cases by narrowing the range of digits used in guessing, and, consequently, increasing the chances for R cases. Tabulations of the digits guessed by the individual reagents were made, and it was found that "but 7 reagents out of the 26

[128] *Vide, supra,* p. 201.

[129] It includes also the per cent of R cases due to subliminal impression. But since the per cent of R cases due to mental habit would be less than 0.8 too great, for the sake of simplicity this fact may be disregarded.

[130] *Supra,* p. 207.

[131] *Supra,* p. 207.

were free from those self-imposed limitations." A correction of the probability of occurrence was applied to each reagent's results, in accordance with the range of his guesses, and the revised results were put in Table LXIX.[132]

This correction, however, did not take account of number-preferences within the range of guessing, and it may fairly be asked whether a common number-habit is not responsible for the significant excess of R cases.

It has been shown[133] that when a considerable number of reagents guess the digits in the digit-series, or the number of spots on playing-cards, their aggregate guesses reveal an excess of guesses on the intermediate digits.

Professor Minot, upon the basis of a study of 8600 guesses, said:

> Calculating from our data, if 10,000 single digits be written down in random order, but with the general intention of putting each digit down the same number of times, we should expect [distribution *A*, in Table LXXX].[134]

Our 10,000 cases of the guessing of spots on playing-cards, by normal reagents, are shown in distribution *B*.

TABLE LXXX.

Digit	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*0*
A	85.4	103.8	112.0	109.4	110.3	108.5	98.8	94.5	92.6	83.3
B	79.3	100.9	111.5	114.2	120.8	110.2	104.3	88.8	88.0	82.0
Avg.	82.4	102.4	111.8	111.8	115.6	109.4	101.6	91.7	90.3	82.7

On the basis of the number-habits common to 110 persons, shown in the *Avg.* above, we may expect, in 1000 guesses of digits, an aggregate of 521.7 (52%) upon the digits presented in our tachistoscopic experiments. The most probable per cent is 50, which leaves as the influence of a common number-habit 2%. Since there were five presented digits, one-fifth of these would contribute R cases, making 0.4% R cases. This contribution by mental habit, in comparison with the excess of 5.44% R cases shown in Table LXVIII, is negligible.

In the other principal series of tachistoscopic experiments, in which the reagents limited their range of guessing to the characters presented, there was no opportunity for the mental habits of the reagents to disturb the chance occurrence of R cases.

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[132] *Supra*, p. 208.

[133] *Supra*, pp. 269-270; also Curves *D* and *E* in Plate XXIX, p. 269, and Plate XXX, p. 270.

[134] *Proceedings Am. S. P. R.*, 1886, Series I, 1:93.

The theory of probabilities is at bottom only common sense reduced to calculus. . . . —LAPLACE: A Philosophical Essay on Probabilities. New York, 1902, p. 196.

The doctrine of chances, in fact, is at the bottom of all scientific argument. . . . —PROFESSOR CHARLES RICHET, Paris, *Proceedings S. P. R.*, 1889, 6:67.

Psychical Research must . . . be made a question of statistics if further conclusions are to be based on the results.—NORTHCOTE W. THOMAS: Thought-Transference. London, 1905, pp. 176-7.

INDUCTIVE PROBABILITY.

When the writer worked out the conditions for the experiments on thought-transference some four years ago he provided for "control" (Card not Imaged) experiments which would occur at random and, so far as chance would allow, would be equal in number to the regular (Card Imaged) experiments, in order that he might have an empirical or Inductive Probability with which to compare the results of the regular experiments.[135] His casual acquaintance with the large deviations in the chance occurrence of events, in a short series of trials, from the probability of a single event, led him to view with distrust the application of theoretical probability. His experience, however, in the last few years, in inspecting and interpreting the experimental results in sets of trials both large and small, has removed his doubt entirely, and has substantially increased his appreciation of the value, to scientific research, of the mathematics based upon the theory of probability.[136]

Instead, then, of essaying an elaborate and detailed exposition of the lawlessness of empirical deviations from theoretical values, calculated to justify a plea for the use of empirical or inductive probability, the writer shall content himself with portraying some of the evidence which has grown out of his work which justifies at once his faith in the use of theoretical probability and his conviction that here lies a means so safe and sure for *proving* the supernormal capacities of thought-transference,

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[135] *Vide, supra,* pp. 35, 50, 147.

[136] The general reader may be referred to the Encyclopædia Britannica for an excellent exposition of the theory by F. Y. Edgeworth; it will be found under the title of "Law of Error" in the Tenth Edition, vol. XXVIII, or of "Probability" in the Eleventh Edition, vol. XXII. The reader with some training in mathematics may be referred, for the development of the theory, to "A History of the Mathematical Theory of Probability," Cambridge and London: Macmillan and Co., 1865, by I. Todhunter. There he will find the development of the formulæ bearing the names of Bernoulli, Poisson, and Bayes, which we have ventured to appropriate [*supra,* pp. 80 ff.]; the more pertinent Articles being 993-997 (pp. 548-558), in the chapter on Laplace. Concerning Laplace's development of "Bernoulli's Theorem" to the form which we have used [*supra,* p. 80], Todhunter said (pp. 552-3): "The result which has just been obtained is one of the most important in the whole range of our subject. There are two points to be noticed with respect to the result. In the first place, it is obvious that supposing γ to be constant we may by sufficiently increasing n render the limits $p-L$ and $p+L$ as close as we please, while the correspond-

lucidity (clairvoyance), or communication from discarnate personalities who can become aware of anything that occurs in our world, that by its persistent use the controversies concerning these alleged phenomena must ultimately give way to universal agreement.

Empirical and Theoretical Distributions.

Let us consider some of the correspondences between our inductive and the theoretical probability. And we may as well begin with the distributions of R cases. If the R cases resulting from chance are distributed about the most probable number in accordance with the theoretical law of chances, then they may safely be predicted from theoretical probability.[137]

In Plates V (*supra,* p. 99), VI (p. 99), VII (p. 101), and VIII (p. 102), in which the distributions of R cases on the Card, Color, Number, and Suit, respectively, in the guessing of playing cards when the cards were not imaged by the experimenter (agent), are represented in solid curves, we may see that although, owing to the relatively small number of cases plotted (100), the curves are irregular and serrated, they follow the general course of the light curve representing theoretical probability, just as the curves representing the distributions of R cases in the "Card Imaged" experiments do. The modes (the abscissae of greatest frequency) and the limits of the curves agree very well. The "Card not Imaged" curves fit the theoretical curves as well as do the "Card Imaged" curves, and for the latter the "closeness of fit" was calculated from

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ing probability is always greater than $\Phi(\gamma)$. [*Vide, supra,* p. 81, bottom of footnote 147 for the value of this expression]. In the second place, it is known that the value of $\Phi(\gamma)$ approaches very near to unity for even moderate values of γ:

γ	$\Phi(\gamma)$
.5	.5204999
1.0	.8427008
1.5	.9661052
2.0	.9953223
2.5	.9995930
3.0	.9999779."

For the use of other statistical formulæ, based upon the theory of probability, and for their vindication by empirical results, the reader may be referred to "An Introduction to the Theory of Statistics," London: Griffin and Co., 1916, by G. Udny Yule (especially Chapters XIII, XIV and XV); and to the first two chapters of Vol. I in "The Chances of Death," by Karl Pearson, London: Arnold, 1897.

[137] The theoretical distributions have been derived by the use of formulæ given in footnotes 166 and 167, pp. 95-6, *supra.*

Pearson's formulæ [138] and presented at the bottom of page 108, *supra*. Of 1000 such distributions we should expect a fit as close as that shown in Plate V in 169 cases, in Plate VI in 84 cases, in Plate VII in 760 cases, and in Plate VIII in 274 cases.

In some of the distributions shown in our study of mental habit the empirical probability curves follow the theoretical curve very closely compared with the deviations of the curves representing the "Card Imaged" experiments, caused by the influence of mental habit upon guessing. This may be seen in Plate XXXIII, (p. 274), which gives the distributions "of the number of times a *number* was drawn or guessed in 100 experiments" in card-guessing; in Plate XXXIV (p. 275) which presents the same data in sets of 1000; and in Plate XXXIX (p. 280), which presents analogous data concerning the occurrence of the *individual card*. In other distributions, Plate XXXV (p. 276), showing the data of the occurrence of a *red card*, and Plate XXXVII (p. 278), showing the data of the occurrence of an individual *suit*, neither of the empirical curves departs significantly from the theoretical curve.

For a significant deviation, for which the "closeness of fit" was calculated, the reader is referred to Plate XI (p. 111) which compares the distribution of R cases on the "Card," expected by the reagents after their experiments had been finished, with the theoretical distribution. The "closeness of fit" was $\Delta = 11.66$ (*vide*, p. 110, *supra*) and the probability of its occurrence by chance was found to be about twice in one septillion sets of 5200 guesses each. Suppose the distribution here compared with the theoretical distribution had been a distribution of R cases actually made in the card-guessing rather than of the number of R cases expected by the reagents, could its remarkable deviation be accepted as proof of an extra-chance cause? This query may well occur to the mind of the reader who recalls the quotation we have made from Venn [139] or who takes theoretical probability at its word and reflects that since this deviation *must* occur by chance twice in one septillion times, this might be one of those times. We might grant, on the basis of theory, that this is indeed the case; that the one highly improbable deviation proves nothing; and that for the purpose of proving an extra-chance cause we must have a plurality of highly improbable deviations. But, theoretically, no matter how large our accumulation of such deviations, there is still

[138] Since the Δ formula was misprinted in footnote 179, p. 110, *supra*, it is given here: $\Delta = \sqrt{\Sigma \frac{\delta_1^2}{y}}$, in which δ_1 is the difference between the theoretical frequency (y) and the observed frequency (f) of each respective class in the distribution, and Σ is the sign of summation. The probability (P) of Δ is found from the formula correctly given in another footnote on p. 110.

[139] *Supra*, p. 149, footnote 229.

the necessity in a total of cases expressed by a number of a yet higher order that it must occur by chance. We therefore prefer to insist that since our quest is for relative knowledge, not absolute knowledge, improbability of an event is an index of the degree to which, on the basis of theory, we may disregard chance as the cause of that event; and for practical purposes, we may fall back upon experience to justify this course. We assert then, our readiness to accept the one highly improbable deviation from theoretical distribution as "significant," and two or more of them as clinching the matter.[140] Although the ouija board has been credited with dictating some remarkable poetry and repartee,[141] the necessary reproduction, according to Venn, of one of the plays of Shakespeare or of Milton's "Paradise Lost" or indeed of any other of the infinite number of written and unwritten literary masterpieces, has not yet

[140] Venn had something to say on both the improbability of the occurrences he suggested in the quotation above referred to, and the objection that any such occurrence may be the one the laws of chance provide for:

Concerning the production of Milton's "Paradise Lost" by recording the letters drawn by chance from a bag, he said:

"It would take more days than we have space in this volume to represent in figures, to make tolerably certain of obtaining the former of these works (Milton's 'Paradise Lost') by thus drawing letters out of a bag . . ." (*op. cit.*, p. 353). And in a footnote on the same page:

Assuming that there are "about 350,000 letters in the work in question, since any of the 26 letters of the alphabet may be drawn each time, the possible number of combinations would be 26^{350000}; a number which, as may easily be inferred from a table of logarithms, would demand for its expression nearly 500,000 figures. . . . Unity divided by this number would represent the chance . . ." $P = 0.[495{,}247$ zeros$]12$; the printing of which would require a book of about one and a half times the size of "Paradise Lost."

And concerning the objection:

"The most seductive form in which the difficulty about the occurrence of very rare events generally presents itself is probably this.

" 'You admit (some persons will be disposed to say) that such an event may sometimes happen; nay, that it does sometimes happen in the infinite course of time. How then am I to know that *this* occasion is not one of these possible occurrences?' To this, one answer only can be given,—the same which must always be given where statistics and probability are concerned,—'The present *may* be such an occasion, but it is inconceivably unlikely that it should be one. Amongst countless billions of times in which you, and such as you, urge this, one person only will be justified: and it is not likely that you are that one, or that this is that occasion.' " (p. 357).

The case of what has been called the "infinitesimal probability," which applies to a single simple event, must be reserved for separate mention. (*Vide*, pp. 346 ff. *infra*).

[141] *Cf.* Patience Worth; a psychic mystery, by Casper S. Yost. New York: Holt, 1916.

occurred; and it may be recalled that on page 83, *supra*, we intimated that scientific proof is satisfied if $P = 0.9999779$, when absolute certainty requires $P = 1$, which permits a negligible margin of two cases by chance in 100,000.

When the probability of a single occurrence is moderate (say from .01 to .5), experience teaches us that the distribution of R cases due to chance does follow the law of chances; that almost all of the R cases are contained within Yule's practical limit of $p \pm 3\sigma$, and that practically all of them are contained within the scientifically satisfactory limit of $p \pm L$.[142]

If we tabulate, from Table XIII (pp. 56-62, *supra*), the aggregate of the R cases made by each reagent on the "Card," in both the "Card not Imaged" and the "Card Imaged" experiments (as, Reagent, *1*, 7; *2*, 3; *3*, 4; etc.) in the form of a distribution, we shall have, on the assumption that chance was not disturbed in the "Card Imaged" experiments, an empirical chance distribution in which there are 100 cases, $p = .025$, and the number of experiments in a set is 100. It may be compared with a distribution made up of the successive terms in the expansion of the bino-

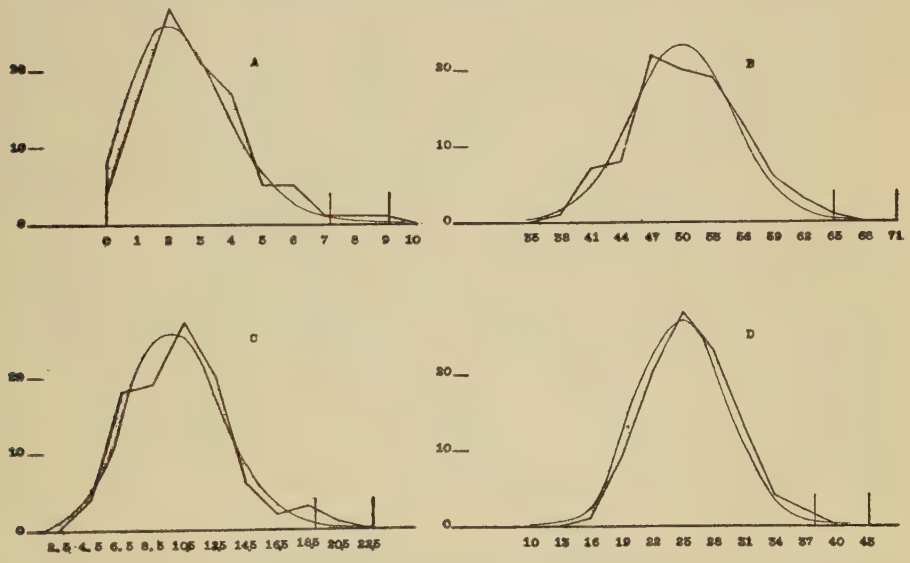

Plate XLII.— Distributions of R Cases in Card-Guessing. Sets of 100. 100 Reagents.

Curve *A*.	On Card,	$pn = 2.5$	$p = .025$
" *B*.	On Color,	$pn = 50.$	$p = .5$
" *C*.	On Number,	$pn = 10.$	$p = .1$
" *D*.	On Suit,	$pn = 25.$	$p = .25$

[142] For the value of the latter limit, *vide, supra*, pp. 82-5; and of the former, the footnote on p. 86, *supra*.

mial 100 $(q+p)^{100}$. It is represented by Curve *A*, in Plate XLII, and the theoretical distribution is represented by the superposed smooth curve.

In like manner we may compare the distribution of the R cases, in sets of 100, on Color, Number and Suit, shown in Curves *B*, *C*, and *D*, with their theoretical curves.[143]

From visual inspection we can see that there is a general correspondence between the empirical and their theoretical curves. We may also note that the empirical distributions in Curves *B* and *D* fall below the 3σ limit (represented by the light vertical line); that in the skewed curves, *A* and *B*, they extend a short distance above the 3σ limit; and that all of them fall below the limit *L* (represented by the heavier vertical line).

By calculation we can give all these facts in arithmetical values:

TABLE LXXXI.

R Cases in Card-Guessing.
100 sets; 100 guesses each.

	Card	Color	Number	Suit
1. $n(p+3\sigma)$	7.2	65.	19.	38.
2. Largest value	9.	64.	20.	37.
3. $n(p+L)$	9.13	71.2	22.7	43.4
4. Δ	4.1	2.66	3.8	2.11
5. P	0.077	0.716	0.165	0.819

By comparing line *2*, which gives the largest value in the empirical distributions, with the line above it, the exact numerical relations between the limit of the empirical distributions and the 3σ limit may be seen; with the line next below it, the *L* limit. Line *4* gives the Pearsonian value of the "closeness of fit," and line *5* gives the probability that the empirical distributions are chance distributions; *e. g.*, in 1000 such distributions, we should expect 77 chance distributions of R cases, on the Card, 716 on the Color, 165 on the Number and 819 on the Suit, to deviate farther from the probability curve than these do. All of these empirical distributions, then, may be taken as good examples of chance distributions—they conform to the theoretical law of chances.

Let us examine some other empirical distributions that are available for our inspection, and in order that we may see that the serrated aspect of our empirical curves in Plates VI-VIII (pp. 99 ff., *supra*) has not been caused by some veiled extra-chance cause, let us first examine the curves

[143] These curves, however, are smoothed somewhat by aggregating the cases on equal inter-classes; *e. g.*, on Color, the cases on each successive three variates are aggregated, those on 49, 50, and 51 being plotted on abscissa 50; on 52, 53, and 54, being plotted on 53; etc. The values in the theoretical curves were found in the same way.

in Plate XLIII. Curve *A* represents the occurrences of the odd numbers in the casting of a die in 100 sets of 100 trials each, distributed upon each successive variate corresponding to the respective terms in the binomial $100(q+p)^{100}$.* Curve *C* represents the occurrences of white balls in 100 sets of 100 drawings from an urn containing an equal number of black and white balls,[144] distributed in the same way. Both curves show the serrated aspect common to our former curves, and for the same reason:

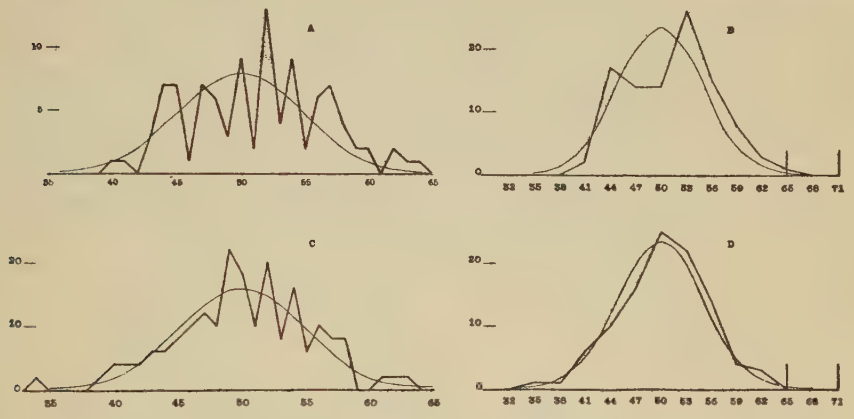

Plate XLIII.— Distributions of the occurrences of Odd Die-spots, and of White Balls drawn from an urn containing an equal number of black and white balls. 100 sets of 100 cases each. $p = .5$.

Curve *A*. Odd die-faces, plotted on the single terms in the series.
" *B*. " " " " " central of three successive terms.
" *C*. White balls, " " " single terms in the series.
" *D*. " " " " " central of three successive terms.

a distribution of a relatively small number of cases over a large number of abscissae. The curves follow in a general way the course of the superposed theoretical curves.

Curves *B* and *D* represent the distributions of the same data, respectively, over successive inter-class values, each composed of three successive variates, or terms in the binomial series. The relations between the empirical distributions and the theoretical distribution are there more apparent. The "closeness of fit" we calculate to be

Curve *B*, $\Delta = 4.30$ $P = .0462$
Curve *D*, $\Delta = 2.36$ $P = .6705$

* These die-casts were made in our 10,000 experiments in card-guessing.

[144] H. Westergaard: Die Grundzüge der Theorie der Statistik. Jena, 1890, p. 22; quoted by Yule: An Introduction to the Theory of Statistics. 3d ed., London, 1916, p. 274.

In 10,000 distributions of an equal number of chance events we may expect 462 to deviate further from the theoretical curve than curve *B* does, and 6705 to deviate further than Curve *D*. It is possible that some even faces of the die were discarded by experimenters who were impatient to have "Card Imaged" experiments, which would introduce an extra-chance cause calculated to shift Curve *B* toward the higher variates as it may be seen to be. But if this occurred, the disturbance of chance was so slight that it cannot be positively identified. One might suspect from the submode on abscissa 44 that some experimenters discarded odd faces; and that the effect of these two opposing influences is shown by the cleft in the center of the curve. But when a distribution contains only 100 cases, and there are nine or ten abscissae over which to plot them, the curves may be expected to be irregular. A cleft in the middle of the distribution-curve is also seen in Curve *A* on Plate XLIV which shows the distribution of the occurrences of a red card in 100 sets of 100 drawings

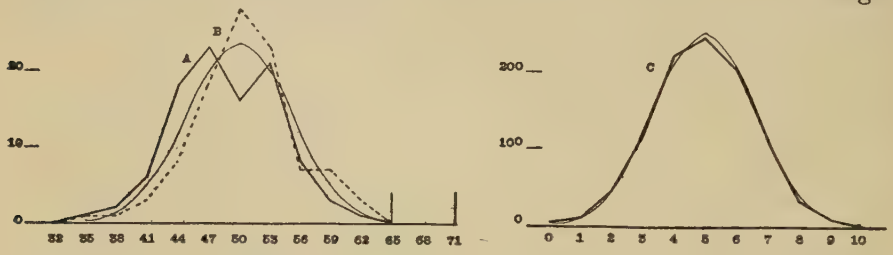

Plate XLIV.— Distributions of the occurrences of a Red Card in drawings from a shuffled pack, and in Guesses of reagents. $p = .5$.

Curve *A*. 100 sets of 100 Drawings each (Stanford)
" *B*. 100 " " 100 Guesses " "
" *C*. 1000 " " 10 Drawings " (Charlier)

each. Since the cards were drawn from shuffled packs containing an equal number of black and red cards, and since no motive on the part of the experimenter can be assigned for discarding any drawn card because of its color, this cleft in the center of the curve must be regarded as accidental, as a chance variation.

If the reader examines the formulæ for the "closeness of fit" and its probability, he learns that they take account of the number of cases included in the distribution. The same fact is illustrated in Plate XLIV. All of the empirical curves on the Plate "fit" their theoretical curves about equally well, although Curve *C* lies much closer than the others to the theoretical curve. In Curve *C* there are 1000 cases, while in Curves *A* and *B* there are but 100 cases. In estimating the degree of congruence between an empirical and its theoretical distribution-curve, the eye cannot be trusted unless the distributions compared include an equal number

of cases distributed over about the same number of abscissae. The "closeness of fit" and probability, for each, are

Curve A, $\Delta = 3.07$, $P = .4908$
Curve B, $\Delta = 3.14$, $P = .4574$
Curve C, $\Delta = 3.04$, $P = .5234$

This is a medium fit; about half of the chance distributions of similar kinds may be expected to fit as close, and half no closer.

Theoretically a run of 100 red cards could take place, so that in the curve one case would be plotted on abscissa *100*, or the run might be on black cards, giving one set in which there were *0* red cards. The chance of this occurrence is $(\frac{1}{2})^{100}$ (or, 0.[30 zeros]789), but it did not take place in any of our sets, just as it has not taken place in any reported results anywhere. The largest number of red cards or of black cards in any set of 100 drawings has not exceeded our scientific limit L (71.2), and very seldom exceeds Yule's practical limit of 3σ (65).

Not all distributions are capable of admitting an exceedingly improbable event, however. Curve C is limited, theoretically, at abscissae *0* and *10*, since the cards were drawn in sets of 10. That all the drawings in a set will be red has a probability of $(.5)^{10}$, or 0.0009766; that is, the occurrence may be expected once in 1024 sets and the same chance holds for a set completely black. Hence in 1024 sets one would expect two of the extreme values. Charlier in his 1000 sets (Curve C) drew no set of all red cards but he drew three sets of *0* red cards. The chance for a set of *1* red card or *9* red cards is 10 in 1024. In Curve C the ordinate value on *1* is 10, and on *9* is 9. In this type of curve, then, which does not admit extremely improbable events, we find the least probable events occurring about as often as the theory of chances calls for.

In Plate XLV we present six curves, all but one of which (Curve B) have, like Curve C, in Plate XLIV, for their limits only moderately improbable events. The upper row shows the results of dice-throwing; the lower, of the drawing of balls in small sets of varying sizes. In Curve A [145] the value of p was $\frac{1}{6}$; in Curve B,[146] $\frac{1}{6}$; in Curve C,[147] $\frac{1}{3}$; in curves D, E and F,[148] $\frac{1}{2}$. This variation in the value of p extends

[145] A Stanford student, Miss Gwendoline Smith, made 1000 throws of two dice, and counted the sixes in each throw. There were only three alternatives: *0*, *1*, or *2* sixes.

[146] W. F. R. Weldon (Encyclopædia Britannica, eleventh edition, vol. XXII, p. 400) made 4096 throws of twelve dice, and counted the sixes in each throw.

[147] G. U. Yule (An Introduction to the Theory of Statistics, 3d ed., p. 259) made 648 throws of three dice, counting both the fives and the sixes in each throw.

[148] Quetelet (quoted by Yule: *ibid.*, p. 274) drew balls from a bag containing an equal number of black and white balls, returning each ball before drawing another. Curves D, E, and F give the distributions of the number of white balls drawn in sets of 5, 6, and 7 drawings.

the range of chance events which we may inspect for concordance between empirical and theoretical probability. In Plate XLII, it is true, p varied, but the events in the distributions might not be regarded as so typical of chance occurrences as the events in Plates XLIII, XLIV, and

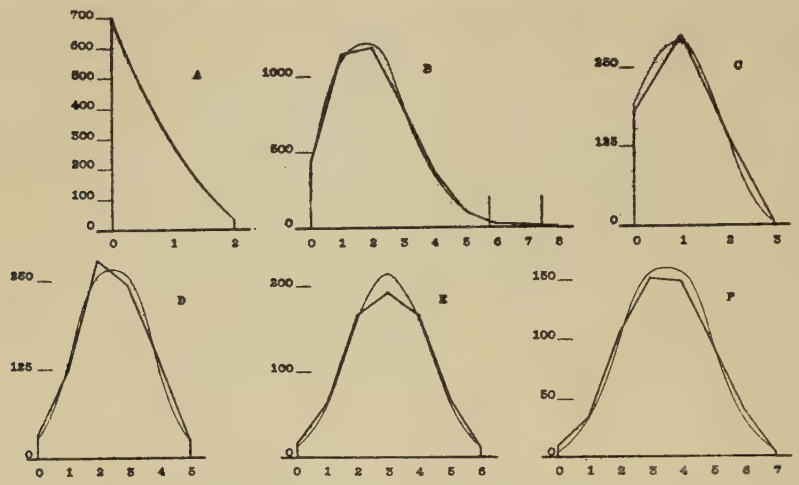

Plate XLV.— Distributions of "chance" events in the throwing of Dice, or drawing of Balls from a bag.

Curve	*A.*	1000	Throws of	2 Dice,	$p = 1/6$	(Miss Smith)
"	*B.*	4096	"	" 12 "	$p = 1/6$	(Weldon)
"	*C.*	648	"	" 3 "	$p = 1/3$	(Yule)
"	*D.*	819	Sets	" 5 Balls,	$p = 1/2$	(Quetelet)
"	*E.*	683	"	" 6 "	$p = 1/2$	"
"	*F.*	585	"	" 7 "	$p = 1/2$	"

XLV; and in the former two $p = .5$. The variation in the size of the sets is also desirable since it extends still further the range of chance events which we may inspect for concordances.

The "closeness of fit" and its probability, for each of the curves are given below:

Curve	Δ	*P*
A	0.355	.99998
B	2.40	.6777
C	1.98	.8660
D	1.94	.8781
E	2.76	.7508
F	2.72	.4956

The fits are close. In 10,000 chance distributions such as that in Curve *F* we may expect 4956 curves to fit the probability curve less well; and in 100,000 such as that in Curve *A*, only two to fit as well.

In Curve *B* there was the possibility of a value on abscissa *12*; *i. e.*, of a set of 12 dice throwing *12* sixes. The probability of the event is

$(\frac{1}{6})^{12}$, or about 0.000,000,000,46; *i. e.*, in about a hundred billion sets we could expect a throw of *12* sixes to occur 46 times. There were 4096 throws and this improbable event did not occur once; nor was there a throw of *11* sixes, or of *10* sixes, or of *9* sixes. The highest number of sixes thrown was *8*, and although the probability is .56 that in 4096 throws of 12 dice *8* sixes will occur, it occurred only once, in close conformance with the theory of chances. There might have been no great surprise had *9* sixes been thrown, since its probability for the given number of throws is .052, and could be expected in five out of a hundred such series of throws.

In Curve *A*, the least probable event was *2* sixes in a throw of two dice. Its probability is $(\frac{1}{6})^2$, or .0278, and could be expected to occur 28 times in 1000 throws. It did occur just 28 times.

In Curve *C*, the least probable event was the throwing of *3* fives and sixes. Its probability is $(\frac{1}{3})^3$, or .038, and could be expected 24 times in 648 throws. It occurred 30 times, only a fourth more often than the number expected.

In Curve *D*, the least probable event was the drawing of *5* white balls or of *0* white balls, in a set of five drawings. In 819 sets it could be expected to occur 25.5 times; *5* white balls occurred *27* times; and *0* white balls occurred 30 times.

In Curve *E*, *0* white balls or *6* white balls could be expected to occur 10.6 times in 683 sets; *0* white balls occurred 17 times, and *6* white balls occurred 8 times. In Curve *F*, *0* white balls or *7* white balls could be expected to occur 4.6 times in 585 sets; *0* white balls occurred 9 times, and *7* white balls occurred 4 times.

The tossing of coins is probably the most satisfactory means of procuring events by chance, because coins are less likely to be subject to extra-chance influences such as the possible differences in surfaces on balls of different colors which might cause the balls of one color to be less slippery to grasp than those of the other color, or the lack of homogeneity in the material of a die or its deviation in form from a perfect cube or the shifting of its center of gravity from the center of the cube by reason of the material milled out of its surface for the spots on the sides bearing the higher numbers. Consequently, some of the students in the writer's class in elementary statistical methods during the past year tossed coins, as well as dice, to procure series of chance events with which to work.

The results of three of these series are presented in Curves *A*, *B*, and *C* in Plate XLVI. The value of p is .5 throughout, and the number of throws in each series was 1000. But for Curve *A* 4 pennies were thrown

at a time;[149] for Curve *B*, 5 pennies; and for Curve *C*, 6 miscellaneous coins. A glance by an experienced eye is sufficient to identify Curve *A* as the only normal curve of the three. This judgment becomes more evi-

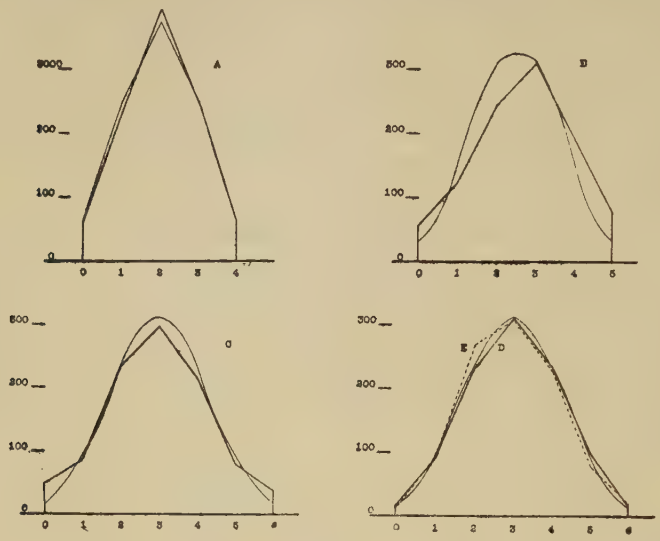

Plate XLVI.— Distributions of "chance" events in Coin-Tossing. $p = .5$.

Curve *A*.	1000	Throws of	4	pennies,	(Miss Sudden)
" *B*.	1000	"	" 5	"	(a student)
" *C*.	1000	"	" 6	"	(a student)
" *D*.	1000	"	" 6	Indian pennies,	(Calvin Clay Coover)
" *E*.	1000	"	" 6	Lincoln "	(J. E. Coover)

dent if the Curves *D* and *E* are included in the comparison, each from 1000 throws of 6 pennies. The "closeness of fit" and its probability follow:

Curve	Δ	*P*
A	1.52	.8880
B	11.1	.000,000,000,000,000,000,000,005,355
C	10.3	.000,000,000,000,000,000,027,7
D (Indian)	0.97	.9965
E (Lincoln)	2.9	.3867

With respect to the least probable events admissible by the curves, in Curve *A*, either *0* or *4* heads could be expected in 62.5 sets; *0* heads occurred 60 times, and *4* heads occurred 62 times. In Curves *C*, *D*, and *E*, *0* or *6* heads could be expected 15.6 times; in *D*, *0* occurred 18 times; *6* occurred 17 times; in *E*, *0* occurred 12 times, and *6* occurred 20 times; but in Curve *C*, *0* occurred 49 times, and *6* occurred 38 times!

[149] This series was obtained by Miss Anita Sudden, who, like the others, counted the number of "heads" in each throw.

It was the shift of this curve toward the lower variates that suggested to the writer the testing of the new Lincoln penny for bias. Examination of the coin seemed to indicate that "tails" was deficient in metal. He accordingly made the 1000 throws of 6 Lincoln pennies, counted the heads in each throw and obtained Curve *E*. For a control series a like number of throws was made with the old "Indian" penny, which seems quite symmetrical, obtaining Curve *D*.[150] Curve *E* is also shifted somewhat toward the lower variates and thus encourages further trials which we intend to make for the sake of illustrating the sufficiency of our method for identifying a slight extra-chance cause quite as much as for settling the matter, yet the amount of shifting is not sufficient to explain the greater shifting of Curve *C*; besides, it was later determined that not Lincoln pennies, but miscellaneous coins, had been used. The excess in the frequency of the least probable events in the latter curve suggests that the extra-chance cause demonstrated by the value of *P* above lies in faulty perception,—incomplete enumeration of heads in general, especially when only one or two heads had been thrown, leading to an excess of *o* heads; and the same oversight with respect to one or two tails, leading to an excess of *6* heads.

A like explanation will not apply to Curve *B*, however. A portion of the series must certainly have been supplied by good intentions in lieu of coin-tossing. Its deficiency in "closeness of fit" might be expected to be equaled by chance in 70 million years if all the inhabitants of the earth (assuming the population to be maintained at 2 billion persons) worked 24 hours per day, throwing five pennies at the rate of a throw every 10 seconds.[151]

But not all the evidence for extra-chance cause in empirical "chance" results can be credited to inexpert performers. And so well have the

[150] The writer is indebted to his wife and his little son, Calvin Clay Coover, for this set. It furnished them an evening diversion for several weeks. But that the counting of heads and the recording were painstakingly done the curve itself bears witness.

[151] To the consternation of the reader who is just now ready to insist that here is a case of the extremely improbable event actually happening, it may be remarked that the author of this distribution had the misfortune, during the preceding year, to be called up before the Women's Council and to be the occasion of diplomatic correspondence between the officials on student affairs. The Curve supplies a neat verification of the dictum pronounced by Frederick W. H. Myers:

"There is *no* choice, I say, however simple or arbitrary—not even the choice between heads and tails or odd and even—which the human mind can be trusted to make as impartially as the spun penny or the roulette-ball would make it." (*Journal S. P. R.*, 1899, 9: 118-9).

laws of chance been established in experience that that evidence is recognized and employed in the most authoritative quarters. Curves *A* and *B* in Plate XLVII present such evidence.

Curve *A* shows the distribution of occurrence of fours, fives, and sixes in 4096 throws of twelve dice:[152] and Curve *B* the like occurrences in 6500 throws of twelve dice.[153] Both curves are seen to be shifted toward the higher variates, and owing to the large number of cases plotted

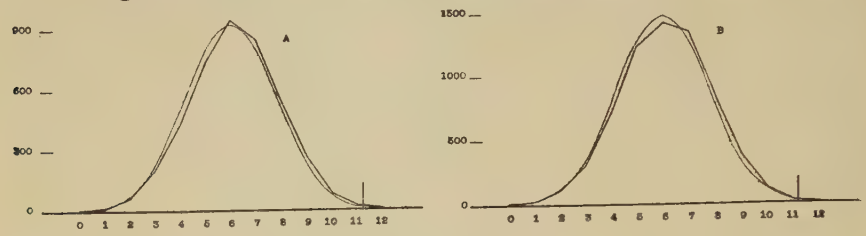

Plate XLVII.— Distributions of "chance" events showing bias in Dice.
4's, 5's, and 6's counted. $p = .5$.
Curve *A*. 4096 Throws of 12 Dice (W. F. R. Weldon)
" *B*. 6500 " " 12 " (A. D. Darbishire)

the curves compare in symmetry very favorably with the theoretical curve. The "closeness of fit" and its probability are found to be

Curve *A*, $\Delta = 5.88$ $P = .00055$
Curve *B*, $\Delta = 6.14$ $P = .00018$

These are the only distributions of a large number of throws of twelve dice, the three higher numbers of which were counted as successes, that have come to the notice of the writer. That they should agree so closely in their deviations from probability strengthens the evidence which each individually presents that an extra-chance cause is responsible for those deviations. Counting the probability of an average "closeness of fit" $P = .5$, the probability of these two fits combined is $2 \times .00055 \times .00018 = .000{,}000{,}198$, which indicates that we could expect only about two such pairs of curves in ten million pairs.

With respect to the frequency of the most improbable events in the distributions, either *0* successes or *12* successes could be expected in Curve *A* to occur once in 4096 throws of twelve dice; neither, however, occurred. One success or *11* successes could be expected 12 times; *1* occurred 7 times and *11* occurred 11 times. In Curve *B*, *0* successes or *12* successes could be expected to occur 1.6 times; *0* occurred 1 time, and *12* occurred 3 times; while *1* or *11* could be expected 19 times, *1* occurred

[152] Made by W. F. R. Weldon (Encyclopædia Britannica, eleventh edition, vol. XXII, p. 394; or Yule: *op. cit.*, p. 258).

[153] Made by A. D. Darbishire (Yule: *op. cit.*, p. 274).

14 times and *11* occurred 21 times. The relation of the complete distribution to Yule's 3σ limit for throws of twelve dice may be seen from the vertical line on 11.2. Thus, although these curves show a slight bias in the dice, owing without doubt to the disproportionate loss of material in the countersinking for the spots on the faces bearing four, five, and six spots, they support the evidence already adduced to show that in frequency distributions the occurrence of the more improbable or highly improbable events conforms very closely to the theoretical law of chances.

In our treatment of the correspondence between empirical and theoretical central measures we shall find this evidence in the distributions supported.

Wherever we see empirical distributions of chance events compared with theoretical distributions, whatever the value of p or the size of the set in which the occurrences are counted, we find that there is a correspondence between the distributions and that the greater the number of events in the series the closer is the correspondence, just as is to be expected on the basis of theory. Karl Pearson[154] has published four remarkable distribution curves which together with our curves illustrate this latter fact, since they are based upon still greater numbers. The first shows the occurrence of red counters in a set of 10 drawings from a bag containing 25 counters each of red, black, yellow, and green; 9148 sets, $p = .25$. The second shows the occurrence of hearts in 10 cards drawn at once from a full pack; 18,600 drawings, $p = .25$. Both of these curves are skewed curves, since the mean number of successes would be a fourth of ten, or 2.5, and while the lowest number of successes possible was but 2.5 below the mean, the highest number possible was 7.5 above it; and both conform so closely to their theoretical curves that to be distinguished from them they had to be represented by a line of dashes between which appeared regularly the dots representing the theoretical curve. The third curve shows the occurrence of fives and sixes in 26,306 throws of 12 dice, ($p = \frac{1}{3}$); it shows on the intermediate variates a slight shifting in conformance with our curves, but owing to the large number of throws plotted it makes a remarkable display of the correspondence of fact to theory. Concerning it Pearson said:

> We see that the two lines are in very close agreement.[155] We may therefore conclude that all the combinations mathematically possible in tossing 12 dice together do actually occur in their due proportions when we throw 12 dice several

[154] Pearson: The Chances of Death. London, 1897, vol. I, p. 13.

[155] "Not such close agreement as occurs in the case of card-drawing and coin-tossing, for dice are never theoretically perfect, and a persistent bias has been observed in them." (Pearson: *op. cit.*, p. 12).

thousand times. This is not a result which we have any right to assume beforehand; that the mathematically possible actually does occur in experiment is demonstrated and can only be demonstrated by actual experience. (pp. 12-3).

The fourth curve shows the occurrence of "heads" in 2048 tosses of 10 shillings ($p = \frac{1}{2}$); and its agreement is also remarkable.

After exhibiting these curves, Pearson said:

When we take a large number of experiments, we see that, however unable we may be to predict the result of a single trial, the frequencies of many trials distribute themselves round the mode in a perfectly orderly manner, and that the law of distribution is precisely that which we obtain by considering all the combinations which might *possibly* occur.

It is not theory, but actual statistical experience, which forces us to the conclusion that, however little we know of what will happen in the individual instance, yet the frequency of a large number of instances is distributed round the mode in a manner more and more smooth and uniform the greater the number of individual instances. When this distribution round the mode does not take place—as, for example, at Monte Carlo—then we assert that some cause other than chance is at work. (pp. 14-15).

We close our illustrations of frequency distributions with Plate XLVIII, which presents in the heavy curve the frequency of runs in

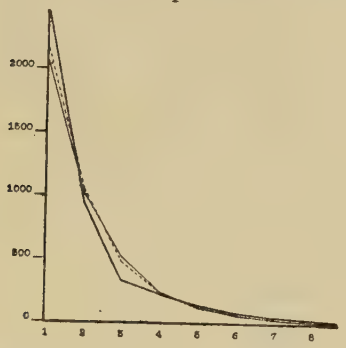

Plate XLVIII.— Runs in Monte Carlo Roulette (heavy line), in Coin-Tossing (dotted line), and to be expected by chance (light line). (Karl Pearson).

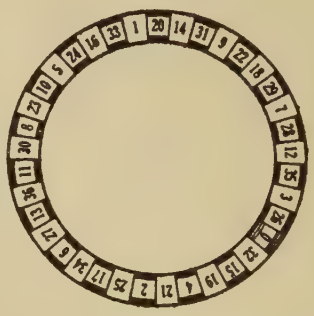

Fig. 5. The arrangement of the 37 compartments in a Monte Carlo Roulette Wheel. The black compartments are partly filled in; the red are left open; zero is green. (After Pearson).

8178 throws of the roulette ball at Monte Carlo; in dotted line, the runs in a like number of tosses of a coin; and in light line, the number of runs to be expected by chance. A run is a permanence in color (in roulette) or in "heads" or "tails" (in coin-tossing) in successive throws; *e. g.*, with *H* for heads and *T* for tails, a sequence of *HTH* is a run of 1 followed by a run of 1 (two intermittences); *HTT* is an intermittence followed by a run of 2; *THHHHH* is an intermittence followed by a run of 5.

Now the roulette is a cylinder containing 37 compartments arranged as is shown in Fig. 5.

This is spun by the croupier, and while it is still rotating a ball is projected in the opposite direction to the rotation on a circular path above the cylinder, and sloping towards its center; from this path the ball ultimately rolls off into one of the 37 compartments. The apparatus is supposed to be made with extreme accuracy, and to be readjusted with the greatest care before the table is used. Admitting the mechanical accuracy of the instrument, and remembering the keen and watchful eyes of the numerous players, it is difficult to conceive a machine better calculated to illustrate the laws of chance than a Monte Carlo roulette.[156]

To it Professor Pearson accordingly turned for illustrative material for popular lectures on the laws of chance:

In my enthusiasm Monte Carlo appeared to me in a new light; it was clearly a scientific laboratory preparing material for the natural philosopher. How to obtain this material in a workable form was the next problem. To spend several months at Monte Carlo recording the spin of the roulette was personally an impossibility, nor did it seem likely that the Royal Society or the British Association would award a grant to pay the expenses of an agent engaged in such a novel form of scientific investigation. Luckily, however, further inquiry led to the discovery that the records of the tables are published in a special journal entitled *Le Monaco,* and issued weekly in Paris at the price of a franc.

He tabulated the 16,500 throws for four weeks in July and August, 1892, and had another similar amount tabulated for him, making 33,000 "chance" events. He first determined the relative occurrence of red and black, which we reserve for later treatment; then he determined the frequency with which the several numbers occurred, his further consideration of which we shall presently quote, and said:

At this result I felt somewhat taken aback. I did not immediately assume that the laws of chance did not apply to Monte Carlo roulette, but I considered myself very unfortunate to have hit upon a month of roulette which was so improbable in its characteristics that it would only occur, on the average, *once* in 167,000 years of continuous roulette-playing. Such were clearly not the most suitable returns for illustrating the laws of chance! (pp. 52-3).

Not wishing to put aside as useless my very improbable month's returns I determined to treat them in another manner; namely, to investigate how closely the runs, that is, successions of numbers, of the same *color,* were in accord with theory. (p. 53).

The values obtained are shown in the above curve as far as a run of 8 colors. The standard deviation (σ) was calculated for each run and with it were compared the deviations of the empirical from the theoretical

[156] Pearson: The scientific aspect of Monte Carlo roulette, in "The Chances of Death," vol. I, p. 47.

frequencies. The runs in a control series of coin-tossing, obtained by Mr. Griffith, were similarly treated. The results follow:

TABLE LXXXII.

Runs	*1*	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*
σ	33	28	22	16	12	8	6	4	3	2	1.5	1
Roulette	325	123	201	47	1	14	10	13	4	3	3	0
Coin-tossing	68	8	45	22	11	3	7	1	4	1	2	0

Professor Pearson observed:

Whenever an actual deviation reaches three to four times the standard deviation, we are approaching the very improbable. [Our limit *L*, it will be remembered, is $4.24 \times \sigma$]. In the case of the tossing, the actual deviation is slightly over *twice* the standard deviation on two occasions [on runs 1 and 3]; in the case of the roulette, on one occasion the actual deviation is nearly *ten* times the standard deviation, on another occasion *nine* times the standard, on a third occasion *four* times, and twice it is *three* times it. *The odds are thousand millions to one against such a deviation as nine or ten times the standard.*[157] If Monte Carlo roulette had gone on since the beginning of geologic time on this earth, we should not have expected such an occurrence as this fortnight's play to have occurred *once* on the supposition that the game is one of chance. My doubts as to the applicability of theory to predict the averages in Monte Carlo roulette were now fairly aroused, but I determined to get, if possible, independent confirmation of my results. My pupil, Mr. L. Giblin, tabulated for me the runs of a second fortnight's play, with the result that his fortnight was so improbable that it was only to be expected once in 5000 years of continuous roulette. Nothing like as bad a fortnight as mine, but quite inconsistent with a reasonable man's applying the laws of chance to Monte Carlo roulette. Finally, Mr. de Whalley investigated 7976 throws of the ball, forming a fortnight's play at a slightly later date than my returns. There resulted deviations 4.63, 4.62, and 4.44 times the standard deviation, or odds of upwards of 263,000 to 1 against such a result. That *one* such fortnight of runs should have occurred in the year 1892 might be looked upon as a veritable miracle; that three should have occurred is absolutely conclusive. Roulette as played at Monte Carlo is not a scientific game of chance.

Such results as those published in *Le Monaco* give the deathblow to the mathematician's theory of the random spinning of a mechanically accurate roulette. The man of science may proudly predict the results of tossing halfpence, but the Monte Carlo roulette confounds his theories and mocks at his laws!

It remains, if possible, to localize the exact points in which Monte Carlo roulette rebels against theory. Mr. de Whalley has kindly tabulated for me the runs of odd and even numbers in 4052 throws, with the result that the actual deviation is on *only one occasion* larger, and then only very slightly larger, than the standard. Thus we see that the totals of red and black and the succession of odd

[157] "The odds against a deviation even six times the standard deviation are more than a thousand million to one!"

and even numbers, are obedient to the laws of chance; the special numbers themselves are in all probability occasionally very chaotic; *the succession of reds and blacks, however, sets the laws of chance at defiance in the most persistent and remarkable manner.*

The abnormal character of these results may be clearly summed up in the words, "superabundance of intermittences and deficiency of small permanences." *Short runs are deficient, and the color changes much more frequently than the laws of chance prescribe.* (pp. 54-7).

In the tabulation of the occurrences of the respective roulette numbers in 16,563 trials Professor Pearson found that:

The deviations from the average in the distribution of the individual numbers are *less* than we should expect. There is a tendency to come nearer the average than the laws of chance would allow of; the totals, to be paradoxical, are too near the most probable result to be themselves scientifically probable. . . . The reader cannot be too often reminded that what is popularly termed "chance" may be chaos or it may be design, but it cannot be scientifically chance *unless the improbable happens in its due proportions.*[158] The absence of the improbable, the redundancy of the probable, is just as much conclusive evidence against conformity with scientific law as the too frequent occurrence of the improbable itself. (pp. 60-61).

To sum up, then: Monte Carlo roulette, if judged by returns which are published without apparently being repudiated by the Société, is, if the laws of chance rule, from the standpoint of exact science the most prodigious miracle of the nineteenth century. Yet even the supernatural would be discredited by fortnightly recurrences; we are forced to accept as an alternative that the random spinning of a roulette manfactured and daily adjusted with extraordinary care is not obedient to the laws of chance, but is chaotic in its manifestations. (p. 61).[159]

If we apply to the empirical curves in Plate XLVIII the formulæ for "closeness of fit" and its probability, we get

Coin-tossing, $\Delta = 4.25$ $P = .1142$
Roulette, $\Delta = 13.7$ $P = .[42 \text{ zeros}]1355$

And if we inspect the distributions for the occurrence of the least probable events we find that, although there was opportunity for a run of *8177*

[158] Italics are inserted by the writer.

[159] Professor Pearson tells us in a footnote (*op. cit.*, pp. 57-8) that since the above was written, he has investigated the runs of color at various places and times: (1) in 18,355 coups in *Rouge et Noir*, made at Saxon-les-Baines some thirty years earlier, he found the distribution of runs "perfectly normal." (2) In "several thousand" coups in Roulette, made at Monte Carlo in 1885, he found the runs "slightly improbable." (3) In 31,074 coups in roulette, made in October and November 1887, at Monte Carlo, he found "a deviation in intermittences of 273, and this equals 3.6 times the standard deviation (75.71)," the odds being "5000 to one against such an excess of intermittences. We may conclude, therefore, that the Monte Carlo returns were beginning to be mathematically improbable in 1887, although they were not then so miraculous as in 1892."

sequences of a single color, as well as for a run of each smaller number down to *13*, none of these highly improbable runs were made. The highest number of sequences was *12*, which occurred in accordance with theory just once, in the roulette series, as it occurred just once in the coin-tossing series.

The concordance of the runs obtained in coin-tossing, shown above, with the runs prescribed by the laws of chance, is matched by the concordance of fact with theory in a more complicated treatment of 4096 throws of a penny made by Miss Alice Johnson.[160] She divided her whole series into "cycles" in which the first event was an *H* following a *T*, and the final event was the last *T* preceding an *H*; thus, her first 17 throws were divided into the following 5 cycles; *HHHTT, HT, HHT, HTTT, HHT*. There was a total of 1014 cycles. These she tabulated and put in a table showing: "(1) The number of runs of different lengths of both alternatives *H* and *T*, and (2), in the case of one alternative, *H*, the runs of each length subdivided to show what length of run of the other alternative they were followed by." Each entry in the table is accompanied by its theoretical value as derived from the law of chances. The general concordance of the empirical values with the theoretical is striking, even for the less probable events. There are nine events that are to be expected but once; they occurred *0, 2, 1, 1, 1, 1, 0, 0, 0*, times, respectively. Eight events are to be expected twice; they occurred *0, 0, 1, 3, 2, 1, 2, 2*, times, respectively. Theoretically, the highest run of *H*'s or *T*'s is *12*; the highest run of *T*'s was *10*, of *H*'s, *15*. For the latter event $P = .0000298$; it could be expected to occur 298 times in ten million throws, or once in about 33,500 throws. Another highly improbable event recorded in the table is a run of *8 H*'s succeeded by a run of *8 T*'s: $P = .00001526$; it could be expected to occur once in about 66,000 throws, and is slightly less probable than the scientifically acceptable limit of chance deviation ($P = .0000221$).

Miss Johnson observed that her table of complex events

> . . . is a further illustration of the fact that the larger the series, the smaller in proportion become the deviations from theory. . . . [The] closer correspondence with theory is only found in the case of events having a comparatively large probability. . . .
>
> Another feature which is rather surprising at first sight is the exact correspondence with theory of some among the very long cycles. It will, however, be seen on consideration that, although the relative deviation from probability in the case of these very large cycles is likely to be often great, the absolute deviation is likely to be almost always small, so that there is really a greater chance of such exact correspondences occurring than in the case of the shorter cycles. . . . (p. 190).

[160] Alice Johnson: Coincidences. *Proceedings S. P. R.*, 1899, 14: 188 ff.

And she concluded:

> If, then, we were trying to judge whether there was evidence of any agency beyond chance in an actual series of this kind, we should have to be very cautious in drawing conclusions from these apparently remarkable occurrences, unless they were very numerous. We should be on much safer ground in dealing only with the deviations from the theoretical results shown by the shorter cycles. Similarly, a few extraordinary coincidences, though much more striking to the imagination, afford much less reliable evidence of something beyond chance than a large number of trivial ones all pointing in the same direction. (p. 191).

It seems reasonable to conclude from this exhibit of the correspondences between empirical and theoretical distributions (1) that, when the value of p lies within the range of .02-.98, chance events empirically derived distribute themselves about the most probable number in accordance with the mathematical law of chances, (2) that the least probable events permitted by the distribution conform in their frequency to this generalization, (3) that the highly improbable events may be practically disregarded, as, indeed, the theory of chances teaches, and (4) that the available formulæ for testing a distribution of empirical events for extra-chance disturbance, which have been sufficiently verified in experience to be confidently employed by the foremost scientists, may be safely and profitably employed in the field of psychical research where it is especially essential to determine whether alleged extra-chance causes are present.

Empirical and Theoretical Central Measures.

When a number of measures are reduced to an average, or the arithmetical mean (M), it is customary to accompany this central measure with a value which indicates its reliability. Sometimes the latter value, to be regarded as a coefficient of precision, is given in the form of the mean variation (MV), which is the average of the deviations of the individual measures from their mean, but more generally it is given in the form of the "probable error" (PE),[161] which is a deviation from the mean that may be expected to include one-half of the means found from any number of further series of measurements of the same thing. Now, this more generally used coefficient of precision is based upon the empirical standard deviation (σ)* and relates the mean to the distribution of the values from which it is obtained. It is an application of the law of

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*$\sigma = \sqrt{\frac{\Sigma\delta^2}{n}}$, in which δ is the deviation of the individual measure from the mean, Σ is the sign of summation, and n is the number of measures in the series averaged.

[161] $PE = .6745 \frac{\sigma}{\sqrt{n}}$.

chances to an empirical measure for the purpose of indicating the precise range ($M-PE$ to $M+PE$) within which the true (but unknown) mean may be expected with a probability of .5 to lie. It is derived from the standard deviation because the latter has become the customary "measure of dispersion" [162] of empirical measures, by the use of which any deviation from the mean may be related to the distribution, and its probability determined.

When the empirical measures are obtained by chance, as in coin-tossing, their dispersion is known to be the result of the errors due to "simple sampling," [163] as dispersion is assumed to be in the other empirical measures, and the standard deviation (σ) as a measure of dispersion becomes the "standard *error*," [164] an instrument for the determination of the probability of the presence of an extra-chance cause. The mean is known, being determined *a priori* by np. When an empirical deviation from the mean is greater than 4.24 times the standard deviation, it is regarded as exceeding the limit of chance deviation (L),—as falling beyond the theoretical distribution of chance events. When it is less than 4σ, its probability by chance can be determined from a table of the probability values arranged for x/σ.[165] Thus we have a definite means based upon the law of chances, for testing empirical central measures for the determination of extra-chance influence. It is part and parcel of the general statistical procedure everywhere in use in the established sciences. Should some procedure based upon inductive probability be claimed to be more applicable than this in the field of psychical research, a presumption would thereby be made that psychical research differs in some essential way from other scientific research, and the claim could be maintained only by establishing this presumption. Practically such a claim can be

[162] Yule: An Introduction to the Theory of Statistics. 3d ed., pp. 142 ff.

[163] *Vide,* Yule: *Ibid.,* pp. 254 ff.

[164] *Idem,* p. 267. The theoretical "standard deviation" is: $\sigma = \sqrt{pqn}$, if expressed in absolute numbers; $\sigma = \sqrt{pq/n}$, if expressed as a ratio of n.

[165] This procedure assumes a symmetrical distribution (*i. e.,* $p = q = .5$) and introduces an error that becomes greater as the value of p departs from the value of q, but that is practically negligible down to $p = .025$, as may be seen by reference to Plate V (p. 99, *supra*), $\sigma = \sqrt{pqn} = \sqrt{.025 \times .975 \times 50} = 1.105$, $3\sigma = 3.315$, and $M + 3\sigma = 4.565$. The distribution is almost wholly below this (Yule's) rough limit; and, as may be learned from Table XLI (p. 89, *supra*), the scientific limit (L) of deviation (*Vide,* pp. 82 f., *supra*) is 4.96, making $M + L = 6.21$, which is above the whole distribution, since, as is shown in Table XLIII (p. 97, *supra*), the greatest number of R cases in a set was *6*. If greater precision is desired in the determination of the probability of a deviation, Bernoulli's Theorem may be used as illustrated on pp. 80 ff., *supra.*

offset by the exhibition of the concordance between empirical central measures and theoretical expectation.

Before examining our empirical central measures, however, let us review a classical instance of the application of inductive probability to the results of psychical research investigation.

On page 34, *supra,* we said, "But Preyer showed that the deviations from probability upon which Richet had based his conclusions are not only equaled but surpassed by experimental chance (in lottery drawings in series of equal length to that of Richet)," taking Preyer at his own representation. And on page 49, *supra,* we quoted the results of one of Richet's series of guessing upon the suit of playing-cards, the probability of which Edgeworth found to be 0.008; there were 2927 trials and 789 (27%, an excess of 2%) R guesses, from which we calculate the ratio of the excess per cent of R cases to the limit of chance deviation (x/L) to be 0.59, and the probability by the x/σ method to be 0.0073. In 10,000 series, such as Richet had, we should expect only 73 of them to show chance deviations greater than +2%; yet the deviation is only 59% of the limit of chance acceptable to science.

Now, since Preyer's criticism is well known, and since only one of Richet's many series has been referred to in this monograph, the reader might reasonably infer that upon the basis of inductive probability Richet's conclusions, based upon theoretical probability, have been once and for all overthrown. Against this inference the writer wishes to suggest two reasons for caution. In the first place, Richet's more striking results, not quoted or reviewed in this volume because they were obtained in experiments which were not similar to any of ours, are much less probably due to chance than the results of the series quoted. In the second place, Preyer's procedure in making the comparison between Richet's results and the results of lottery-drawings does not seem satisfactory; from his own figures (*op. cit.,* pp. 66-7) the deviations of Richet's results may be shown to exceed those of the lottery results.

Accepting the questionable method of aggregating the results of Preyer's 18 listed series of Richet's experiments in which p varies through 17 gradations from 0.5 to 0.0128, we start, with Preyer, with 8670 cases, 2177 R cases, and 2019 the most probable number of R cases; from the latter we get $p = .233$ (23.3%), and the other values in the first line in Table LXXXIII, *infra.* And taking the average of the R cases per 8670 drawings for the 12 series selected by Preyer from the record of the "konigl. Sachsischen Lotterie" for May 1885, we get 193 or 2.22% as against the probability of 2%, from which we calculate the rest of the values in the 2d line of the table. While the excess of R cases over prob-

ability in Richet's aggregated results comes very close to the limit of chance deviation ($x/L = .910$), and, by the x/σ method, may be expected to be exceeded by only 12 plus and minus chance deviations in 100,000 aggregates such as Richet's, the average excess of R cases in the selected series of lottery drawings amounts to only 34% of the limit of chance deviation, and may be expected to be exceeded by 14,430 plus and minus chance deviations in 100,000 averages.

TABLE LXXXIII.

	n	R	p	x	x/L	x/σ	P
1. *Richet*	8670	.2510	.233	.0180	0.910	3.85	.00012
2. *Lottery*	8670	.0222	.020	.0022	0.344	1.46	.14430
3. *Richet*	657	.2290	.167	.0620	1.005	4.26	.00002
4. *Lottery*	8670	.0235	.020	.0035	0.550	2.33	.00990

If we appeal from aggregates and averages, and inspect the largest deviations in the two tables of contrasted results, we find equally decisive evidence for the superiority of Richet's results. In the table of Richet's results is a series of 657 guesses; R cases $= 22.9\%$, $p = 16.7\%$, excess $= 6.2\%$. The excess, as is shown in the 3d line of the table above, just exceeds the limit of chance deviation (x/L), and is decisive in indicating an extra-chance cause working for R cases. Only 2 chance deviations out of 100,000 such series may be expected to equal or exceed it. The largest deviation in the lottery series is 0.35% ($p = 2\%$), which, as is shown in the 4th line of the table above is only 55% of the limit of chance deviation and may be expected to be exceeded by 990 positive chance deviations in 100,000 series of lottery drawings.

The conclusions drawn by Preyer were based upon a comparison of the x/p values, which, because of the difference in the values of p, was unsound procedure. More comparable values would have been $x/\sqrt{p-p^2}$.

This classical instance of the use of an inductive probability to test the results of experiments in thought-transference fails not only in its specific purpose, but also in revealing any empirical deviations which are not to be expected from theoretical probability, or any characteristics which commend an inductive probability over the theoretical probability as an instrument for testing for an extra-chance cause the results of experiments the conditions of which indicate the probability of a single occurrence.

Let us now examine our empirical central measures of chance events. It will be recalled that while we were examining the *distributions* of empirical chance events we took occasion to inquire whether the highly improbable events occur, and whether the least probable events which

actually did occur occurred with a frequency demanded by theoretical probability. These events were single measures, and our judgment concerning them depended upon their relation to the limiting values of the theoretical distribution. If the largest positive deviation in any empirical distribution was not greater than the laws of chance call for, but was approximately as great, or if the largest possible deviation occurred approximately as often as theory demands, these empirical deviations were regarded as supporting the theoretical law of chances by which they could be safely predicted. When the largest empirical deviations from the mean occurred much more often than theory allowed (as in Curve *B*, Plate XLVI),[166] it was confidently stated that they were disturbed by extra-chance causes, as was justified by the conformance of all carefully acquired empirical distributions with their theoretical distributions. Now, a central measure, the mean, or the per cent of R cases, is a similar single measure which belongs to a distribution of empirical means and can be tested in precisely the same way. Should a mean fall beyond the limit of the theoretical distribution to which it belongs, it would be regarded as being disturbed by an extra-chance cause. Should it fall within its theoretical distribution, its probability can be found just as the probability of any event in a distribution can be found. If the empirical central measures which are available for our inspection are found to conform to theory, both theoretical probability and the formulæ applicable to central measures will receive confirmation.

1. In the guessing of Lotto-Block numbers there were two series of approximately 500 guesses, one of them being a "Number Imaged" series. Assuming that chance causes were undisturbed in the production of R cases, we have in Table III (p. 39, *supra*) twelve empirical central measures of chance events. Each is the mean number of R cases per guess, or the per cent of R guesses. The theoretical mean is the probability of an R case by chance, expressed in per cent; and the difference between these two values we may call x. In the following table we display these twelve empirical measures together with the other values mentioned from which we calculate the probability that they are caused by chance (P).

We note in the x/σ column only one value greater than unity, which, to be significant, should be as great as 4.24. And in the P column we see that the probability is high enough in all cases to exclude the hypothesis of extra-chance cause. The 12 central measures of R cases in the guessing of Lotto-Block numbers conform to the expectation for chance events.

2. In the last column of Table I (p. 38, *supra*) we have ten aggre-

[166] *Supra*, p. 324.

TABLE LXXXIV.

	R	p	x	x/σ	P*
Whole number Number Imaged.....	1.00	1.23	0.23	.47	.638
Number not Imaged..	1.18	1.23	0.05	.0102	.592
Ten's digit Number Imaged.....	12.8	12.5	0.3	.205	.837
Number not Imaged.....	11.1	12.5	1.4	.96	.337
Unit's digit Number Imaged.....	12.2	10.0	2.2	1.5	.134
Number not Imaged.....	9.1	10.0	0.9	.67	.503
Ten's for Unit's Number Imaged....	6.6	6.9	0.3	.265	.791
Number not Imaged	7.4	6.9	0.5	.44	.660
Unit's for Ten's Number Imaged....	6.0	6.9	0.9	.79	.430
Number not Imaged	8.0	6.9	1.1	.97	.332
Transposed Number Imaged.....	1.23	0.85	0.38	.925	.355
Number not Imaged.....	0.80	0.85	0.05	.122	.903

gates of the occurrences of blanks drawn from the bag in 100 experiments; $p = .5$; they range from 41-57; the deviations (x) are -9 to $+7$; x/σ for these limiting deviations is 1.8 and 1.4 respectively, giving a probability (P) of 0.07186 and 0.16152 respectively, that they are chance deviations. These are the central values, for a set of 100 experiments, which deviate most from the theoretical central value. If we consider the central value for the whole series of experiments (1000), 502, or 50.2%, we find $x = 0.2\%$, $x/\sigma = 0.126$, $P = .8997$. All of these empirical central measures, then, fall well within the theoretical distribution, the scientific limit of which, it must be constantly remembered, demands that x/σ is as large as 4.24, or that P is as small as .0000221.

3. In Tables IV and VI (pp. 39-40 *supra*) we have, in the last column, the occurrences of the respective digits, in unit's place, in 500 drawings of Lotto-Blocks from the bag. There are 20 cases; the range is 37-65; $p = 1$; $x = -13$ and $+15$; $x/\sigma = 1.93$ and 2.23; $P = 0.05360$ and 0.02574. These 20 central empirical measures fall well within the theoretical distribution.

4. In Table X (p. 42, *supra*) we have the chance occurrence of the respective die-spots in 500 throws. There are 12 cases; $P = \frac{1}{6}$. The central measures (the number of occurrences) range from 71 to 95 and deviate from the theoretical central measure (83) as much as -12 and $+12$; for these minimal and maximal values, $x/\sigma = 1.44$, $P = .14986$. In Tables LI (p. 126, *supra*) and XXII (p. 70), we have the like occurrences in sets of 1000 and 9800 throws respectively; and if we add to

* The number under P expresses the probability that the x is a chance deviation; *e. g.* the first P value indicates that the probability of the x value of 0.23 being a chance deviation is .638 in 1; or that of 1000 such values 638 chance deviations may be expected to be larger than x. $P = \Sigma_{x/\sigma}^{\pm\infty}$.

the data included in these three tables, those of the experiments on "The Feeling of Being Stared At," we shall have a set of 14,600 throws, in which the die-spots occurred as follows:

1, 2502	*3*, 2491	*5*, 2480
2, 2350	*4*, 2349	*6*, 2428

While the most probable number is 2433. Now, if we calculate the probability (P) of the largest negative deviation and the largest positive deviation of the central measures obtained from these tables, and from the aggregate of throws, we find the values in the following Table, the last line of which gives the theoretical limit of chance deviation (L):

TABLE LXXXV.

Die-throws. $p = 1/6$.

Table	Page	Number of Expmts.	Number of Meas.	$-x$ %	$-x/\sigma$	P	$+x$ %	$+x/\sigma$	P
X	42	500	12	2.4	1.44	.14986	2.4	1.44	.14986
LI	126	1000	6	1.7	1.44	.14986	2.8	2.38	.01732
XXII	70	9800	6	0.87	2.28	.02260	0.50	1.33	.18350
Aggregrate		14600	6	0.58	1.44	.14986	0.47	1.18	.23800
Limit (L)					4.24	.0000221		4.24	.0000221

It will be seen that all of these occurrences, the least probable in our data under consideration, are in accord with theoretical expectation; and that the larger the set, the smaller does the deviation from the theoretical central measure (in per cent) become, and the probability that the empirical deviation is a chance deviation does not decrease.

5. In a similar way we may examine the various empirical central measures of the larger sets of trials which our data provide, together with similar empirical measures from other sources. And, since the value of the probability of a single occurrence (p) is an important consideration, the data will be presented in tables under the varying values of p. In Table LXXXVI the various available sets of data in which the probability of a single occurrence ($p = .025$) is small are brought together; the number of experiments or trials (n), the deviation of the per cent of R cases or occurrences from the theoretical per cent (x), the amount of this deviation in terms of the theoretical standard deviation (x/σ), and the probability that the deviation is a chance deviation (P) are given. Line *1* gives the R guesses on the "Card" in 5135 "Card Imaged" experiments (*vide*, Table XV, p. 64, *supra*), and line *2* gives the R guesses in 4865 "Card not Imaged" experiments, while line *3* gives

the aggregate of these R guesses in the total of 10,000 experiments. Line *4* gives the occurrence of the Ace of Hearts, and line *5* of the Four of Spades, in the drawing of cards from shuffled packs, in our card-guessing experiments; the former occurred less often, the latter more often, than any other card. Line *6* gives the occurrence of Zero in 31,074 coups at Roulette in Monte Carlo play from October to November 1887, as calculated by Pearson [167] from returns published in the semi-official *Le Pointeur*. Line *7* gives the occurrence of Zero during 48 days of play at one table in Monte Carlo reported by "a student of the game of Roulette"; [168] and line *8*, the maximum deviation in the occurrence of a single number during the same period. (In the Roulette data, $p = .027$). The last line in this and in the immediately succeeding tables gives the x/σ and P values for the theoretical limit of chance deviation.

TABLE LXXXVI.

$p = .025$.

Source	Method	Event	n	x	x/σ	P*
1. Stanford	Cards	R Guess	5135	0.48	2.20	.02780
2. "	"	"	4865	0.40	1.79	.07346
3. "	"	"	10000	0.44	2.82	.00480
4. "	"	1 H	10000	0.34	2.18	.02926
5. "	"	4 S	10000	0.50	3.21	.00132
6. Pearson	Roulette	Zero	31074	1.60	11.94	.00000
7. "	"	"	31374	0.166	1.82	.06876
8. "	"	Max.	31374	0.252	2.76	.00578
Limit of Chance (L)					4.24	.0000221

All of these deviations, except that in line *6*, have a probability which places them within the range of theoretical expectation. The least probable is that of the occurrence of the Four of Spades, and it is to be expected 132 times in 100,000 sets of 10,000 drawings. Those in lines *4* and *5* are the largest of 40 deviations, the probability of which would, of course, be higher than the probability of these respective events; and that in line *8* is the largest in 37 deviations, of which that in line *7* is one. These empirical central measures conform to the law of chances. The "student" who contributed the data in lines *7* and *8* found that 17 out of 37 deviations were *0*, and that 10 others were not greater than 0.016%; the deviation of 0.252% was regarded as a noteworthy "exception." The

* *Vide*, footnote to Table LXXXIV, p. 338, *supra*.

[167] Pearson: Scientific aspect of Monte Carlo roulette, in "The Chances of Death," vol. I, p. 57.

[168] *Ibid.*, p. 58.

deviation in line *6*, however, cannot be accepted as a chance deviation (the limit being $x/\sigma = 4.24$). A study of the occurrence of the respective numbers in Roulette led Professor Pearson to say: "the special numbers themselves are in all probability occasionally very chaotic."[169] One series of 16,563 throws which he examined gave him results of which he said: "The odds against a divergence so great as this are roughly about 2,000,000 to 1."[170]

In Table LXXXVII the probability of occurrence is a step higher ($p = .1$). Lines *1* and *2* give the R guesses on the number of spots in the "Card Imaged" and "Card not Imaged" experiments, respectively, (from Table XV, p. 64); line *3* gives the aggregate; line *4* gives the R cases in the guessing of digits, and line *5* in the guessing of spots on playing-cards conducted by the old American Society for Psychical Research;[171] lines *6* and *7* give the occurrences of the 7-spot and 10-spot cards in drawings from a shuffled pack, the former having been less frequent, the latter more frequent, than cards with other numbers of spots.

TABLE LXXXVII.

$p = .1$.

Source	Method	Event	n	x	x/σ	P
1. Stanford	Cards	R Guess	5135	0.477	1.142	.25346
2. "	"	"	4865	0.031	0.072	.94260
3. "	"	"	10000	0.260	0.867	.38596
4. Am. S. P. R.	Digits	"	12130	0.333	1.22	.22248
5. "	Cards	"	4050	0.593	1.26	.20766
6. Stanford	"	7-spot	10000	0.75	2.50	.01242
7. "	"	10-spot	10000	0.51	1.70	.08914
Limit of Chance (L)					4.24	.0000221

All of these deviations are well within the field of chance deviation.

In Table LXXXVIII the probability of occurrence is a second step higher ($p = \frac{1}{6}$ or .167). Line *1* gives the occurrence of the 6-spot in Miss Smith's aggregate of 2000 throws (from the distribution shown in Curve *A*, Plate XLV),[172] line *2* gives the occurrence of the 6-spot in the aggregate of our throws of dice, and line *3* gives the like occurrence in Weldon's throws of 12 dice (from the distribution shown in Curve *B*, Plate XLV).[173]

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[169] *Op. cit.*, p. 56.

[170] *Ibid.*, p. 52.

[171] *Proceedings Am. S. P. R.*, Series I, 1:28, 107.

[172] *Supra*, p. 322.

[173] *Supra*, p. 322.

TABLE LXXXVIII.

$p = 1/6 = .167.$

Source	Method	Event	n	x	x/σ	P
1. Stanford	Dice	6-spot	2000	0.220	0.837	.40260
2. "	"	"	14600	0.030	0.074	.94102
3. Weldon	"	"	49152	0.000	0.000	.99999
Limit of Chance (L)					4.24	.0000221

The probability of the deviation from the theoretical mean in line *1* is close to average, in lines *2* and *3* exceptionally high.

In Table LXXXIX the probability of occurrence is a third step higher ($p = .25$). Lines *1* and *2* give the R guesses on "Suit" in the "Card Imaged" and "Card not Imaged" experiments, respectively, (from Table XV, p. 64), and line *3* gives the aggregate R guesses; line *4* gives the occurrence of Hearts, line *5* of Spades, in the drawings of the cards from a shuffled pack, the former having occurred less often, the latter more often, than any other suit, and line *6* gives the occurrence of one head in throws of four pennies made by Miss Sudden (from the distribution shown in Curve *A*, Plate XLVI).[174]

TABLE LXXXIX.

$p = .25.$

Source	Method	Event	n	x	x/σ	P
1. Stanford	Cards	R Guess	5135	1.17	1.94	.05238
2. "	"	"	4865	0.74	1.19	.23404
3. "	"	"	10000	0.96	2.21	.02710
4. "	"	Hearts	10000	0.68	1.57	.11642
5. "	"	Spades	10000	1.60	3.70	.00022
6. "	Pennies	1 Head	1000	1.60	1.17	.24200
Limit of Chance (L)					4.24	.0000221

Again all the deviations lie within the limits of chance. One deviation, however, that in line *5*, is interesting in that its probability is less than any others to be found among our empirical central measures; yet we should expect the occurrence of a suit at least as often in 22 out of 100,000 sets of 10,000 drawings of cards.

In Table XC the probability of occurrence is a fourth step higher ($p = 1/3$). Line *1* gives the occurrence of the 5-spot and the 6-spot in the aggregate throws of dice, and line *2* gives the like occurrence in 248 throws of three dice made by Yule.[175]

[174] *Supra*, p. 324.

[175] Yule: An Introduction to the Theory of Statistics, 3d ed., p. 259.

TABLE XC.
$p = 1/3 = .333.$

Source	Method	Event	n	x	x/σ	P
1. Stanford	Dice	5 and 6	14600	0.283	0.725	.46846
2. Yule	"	"	1944	1.13	1.060	.28914

These deviations, as their probability shows, are good samples of chance events.

Table XCI displays a number of empirical central measures from sets varying considerably in size and in method; the probability of a single occurrence is .5. Line *1* gives the occurrence of drawing the blank side of a Lotto-Block (from the total of the last column of Table I, p. 38, *supra*); lines *2* and *3*, the largest negative and largest positive deviations, respectively, in the occurrence of an odd die-number in 13 sets of 1000 throws; lines *4* and *5*, the R guesses on color in the "Card Imaged" and "Card not Imaged" experiments, respectively (from Table XV, p. 64); lines *6* and *7*, the like occurrences, respectively, in the thought-transference experiments conducted by the old American Society for Psychical Research;[176] lines *8* and *9*, the occurrence of a red card in a set of 10,000 drawings from a shuffled pack of red and black cards, the former made in our thought-transference experiments, (see Curve *A*, Plate XLIV),[177] the latter made by Charlier[178] (see Curve *C*, same plate); line *10*, the occurrence of one of the first forty-five numbers in drawings made by Westergaard[179] from a bag containing 90 tickets numbered from 1 to 90; lines *11*, *12* and *13* the occurrence of heads in the tossing of pennies at Stanford (of which the distributions are shown in in curves *A*, *D*, and *E*, Plate XLVI);[180] lines *14*, *15*, *16*, *17*, and *18*, the like occurrence in the tossing of coins by Miss Alice Johnson,[181] Buffon,[179] De Morgan,[179] Griffith,[179] and Pearson,[179] respectively; line *19*, the occurrence of heads in the aggregate of the throws reported in lines *11*, *12*, and *13*; line *20*, the like occurrence in throws of a penny made by Jevons;[182] line *21*, the like occurrence in throws of a shilling, made by Pearson;[179] lines *22* and *23*, the occurrence of white balls in the drawings of white and black balls made by Quetelet,[179] and Westergaard,[183] (see

[176] *Proceedings Am. S. P. R.*, 1885, Series I, 1:20.

[177] *Supra*, p. 320.

[178] Arne Fisher: The Mathematical Theory of Probabilities and its application to frequency curves and statistical methods. New York, 1915, vol. I, p. 138.

[179] Pearson: The scientific aspect of Monte Carlo, *loc. cit.*, p. 48.

[180] *Supra*, p. 324.

[181] *Proceedings, S. P. R.*, 1899, 14:189.

[182] John Grier Hibben: Inductive Logic. New York, 1901, p. 236.

[183] Yule: An Introduction to the Theory of Statistics. 3d ed., p. 247.

Curve *C*, Plate XLIII),[184] respectively; line *24*, the occurrence of odd die-spots in the aggregate throws of dice made in connection with experiments reported in PART I; line *25*, the occurrence of die-spots *4*, *5*, and *6*, in the same aggregate of throws; lines *26* and *27*, the like occurrence in throws made by Weldon[185] (see Curve *A*, Plate XLVII),[186] and Darbishire[187] (see Curve *B*, same plate), respectively; line *28*, *29*, and *30*, the occurrence of red in the coups of the Roulette at Monte Carlo, as calculated by Pearson, [179] De Whalley, [179] and Pearson,[188] respectively.

TABLE XCI.

$p = .5$.

Source	Method	Event	n	x	x/σ	P
1. Stanford	Lotto-Blocks	Blank	1,000	0.20	0.126	.89974
2. "	Dice	Odd	1,000	1.80	1.140	.25428
3. "	"	"	1,000	4.10	2.590	.00940
4. "	Cards	R Guess	5,135	0.224	0.321	.74822
5. "	"	"	4,865	1.22	1.680	.09296
6. Am. S. P. R.	"	"	5.500	0.51	0.754	.45086
7. "	"	"	5,150	0.35	0.501	.61638
8. Stanford	"	Red	10,000	1.06	2.120	.03400
9. Charlier	"	"	10,000	0.66	1.320	.18684
10. Westergaard	Tickets	1st half	7,275	0.034	0.058	.95374
11. Sudden	Coins	Heads	4,000	0.45	0.570	.56868
12. J. E. Coover	"	"	6,000	0.62	0.955	.33958
13. C. C. Coover	"	"	6,000	0.38	0.594	.55252
14. Johnson	"	"	4,096	0.22	0.281	.77872
15. Buffon	"	"	4,040	1.00	1.268	.20480
16. De Morgan	"	"	4,092	0.05	0.064	.94898
17. Griffith	"	"	8,178	0.04	0.072	.94260
18. Pearson	"	"	12,000	0.16	0.351	.72560
19. Stanford	"	"	16,000	0.025	0.063	.94976
20. Jevons	"	"	20,480	0.55	1.575	.11526
21. Pearson	"	"	24,000	0.05	0.155	.87782
22. Quetelet	Balls	White	4,096	0.40	0.513	.60796
23. Westergaard	"	"	10,000	0.11	0.220	.82588
24. Stanford	Dice	Odd	14,600	1.185	2.860	.00424
25. "	"	4, 5, 6	14,600	0.295	0.712	.47648
26. Weldon	"	"	49,152	1.16	5.130	.00000
27. Darbishire	"	"	78,000	1.24	6.920	.00000
28. Pearson	Roulette	Red	16,141	0.15	0.380	.70394
29. De Whalley	"	"	16,019	0.27	0.684	.49398
30. Pearson	"	"	30,575	0.0147	0.0515	.95892
Limit of Chance (L)					4.24	.0000221

[184] *Supra*, p. 319.
[185] Yule: *Op. cit.*, p. 258.
[186] *Supra*, p. 326.
[187] Yule: *Op. cit.*, p. 274.
[188] Pearson: *Op. cit.*, p. 57, footnote 2.

All of the entries in this table, excepting lines *26* and *27*, are in accord with the theoretical law of chances; in 100,000 sets, each comparable in number to the respective sets entered in the table, we may expect from 424 (line *24*) to 95,892 (line *30*) to yield deviations from the theoretical central measure greater than those which have occurred in these sets. Evidence for an extra-chance cause is satisfactory if the probability of the deviation is so small as to permit but 2, and this requirement is somewhat more than met in the deviations in lines *26* and *27*, which occurred in throws of dice. The distributions of these sets were found[189] to deviate sufficiently from the theoretical distribution, that, together, they could not be expected to occur once in a million pairs of distributions. In the x/σ column it may be seen that their deviations are 5.13 and 6.92 times the theoretical "standard deviation," while the limit of chance prescribes 4.24 times that measure. Of the deviation in Weldon's throws ($x/\sigma = 5.13$) Yule said:[190]

> The deviation observed is 5.1 times the standard error, and, practically speaking, could not occur as a fluctuation of simple sampling. It may perhaps indicate a slight bias in the dice.

And concerning a deviation of 6σ, Karl Pearson said:[191]

> The odds against a deviation even six times the standard deviation are more than a thousand million to one.

Karl Pearson has also observed,[192] concerning dice, that "a persistent bias has been observed in them."

These negative instances tend to strengthen the main body of evidence adduced to show the concordance of empirically derived central measures with theoretical expectation, and also to support the thesis that in the theory of probability and the mathematical formulæ derived therefrom lies an instrument of precision capable of revealing the presence of even slight extra-chance causes and worthy of more generous adoption in the field of psychical research. If the slight bias of dice can be demonstrated, other slight constant causes certainly can.

In this section we have examined the deviations of empirical central measures from theoretical central measures for an aggregate of 2,214,622 experiments in sets both small and large and when the probability of a single occurrence of a designated event varied within the limits of $p = .025$ and $p = .5$, and, except for the few cases accounted for, we have

189 *Supra*, p. 326.
190 Yule: *Op. cit.*, p. 267.
191 *Supra*, p. 330, footnote 157.
192 *Supra*, p. 327, footnote 155.

found that the empirical central measures derived under all these varying conditions, do not deviate from the theoretical central measures farther than theory prescribes. Both theoretical probability and the formulæ derived therefrom which are applicable to central measures have received confirmation.

The Infinitesimal Probability.

When considering the application of mental habit to experiments on thought-transference we noticed the unsatisfactory nature of the conditions of experiment which permitted the unreckoned influence of common associations, resulting from "psychical communism," [193] and observed that the infinitesimally probable upon which reliance is placed may in reality be highly probable. Again, when considering Venn's statement that any given highly improbable event (*e. g.*, the production of Milton's "Paradise Lost," by recording the letters drawn at random from a bag) must certainly occur if the trials are only kept up long enough, and his counter-assertion, to the claim that any given empirical event may be one of the occurrences which theory prescribes, "it is inconceivably unlikely that it should be one," we reserved for separate consideration the "infinitesimal probability." [194] It is now time to see what we can make of it.

We have found that the distribution of frequency values derived from empirical chance events agrees, within the limits prescribed by the law, with the law of chances, and that when the empirical value under consideration is the occurrence of a least probable or a highly improbable event, or is a central measure, the empirical value falls within the prescribed limits in the theoretical distribution, also in accordance with the law of chances. If the event is highly probable, such as 25 "heads" in 50 throws of a coin, it occurs more often in a given number of sets of 50 throws than does 26 heads, 27 heads, 30 heads, 40 heads or 50 heads. The law of chances gives, for these events, the following probabilities:

Heads	*P*
25	.112
26	.108
27	.096
30	.0419
40	.000,009,12
50	.000,000,000,000,000,888

The empirical frequency values follow, as we have found, the theoretical

[193] *Supra*, pp. 297, 305-8.

[194] *Supra*, p. 316, footnote 140.

frequencies very closely. If we have 100 sets of 50 throws we may expect about 11 or 12 sets to yield *25* heads, about 10 or 11 to yield *24* or *26* heads, about 9 or 10 to yield *23* or *27* heads, about 4 to yield *20* or *30* heads, none to yield *10* or *40*, or *0* or *50* heads. If we had 100,000 sets, we could expect about one set to yield *10*, and another set to yield *40*, heads. If we had 1,000,000,000,000,000 sets, we should not be surprised to find one set with *0* heads, or another with *50* heads, or both. In other words, we expect the events of varying probability to occur in their proper ratio.

Whether any given number of heads in a set of 50 throws is a highly improbable event consequently depends upon the number of sets to be examined.

The same reasoning applies to R guesses; for example, on cards drawn from a shuffled pack of 40. The probability of a single occurrence ($p = .025$) is much less than in the case of the coin-tossing ($p = .5$), and the probability of the various numbers of R cases in a set of 50 guesses, is much different:

R Guesses	*P*
0	.282
1	.362
2	.227
3	.093
4	.028
5	.007
6	.001
7	.000,2
8	.000,03
9	.000,003
10	.000,000,4
19	.000,000,000,000,000,05

If 100 sets of 50 guesses are examined, we should expect to find only about one set in which there are 5 R cases, and would not be surprised to find 6 R cases, which should occur about ⅙ as often as 5 R cases. We could even look upon 7 R cases as a chance event. But 8 or more R cases would be regarded as evidence of an extra-chance cause. If we had 100,000 sets we could expect about 3 sets giving 8 R guesses, and probably one giving 9 R guesses. If we had 1,000,000,000,000,000 sets we might reasonably expect a set or two giving 18 R guesses, but we would not expect any giving 19 R guesses. Thus, here too, one expects the events which have varying chances for occurrence to occur in their proper ratio. And, because of the difference in the value of p, we should expect, in a tremendous number of sets of 50 trials, 19 R guesses on the

"Card" less often than 50 R guesses on the Color of the card (when $p = .5$, the same as for heads in coin-tossing).

Our survey both of empirical distributions and of empirical central measures has justified the expectation that whatever the value of p, or whatever the number of the trials or sets of trials examined, the various frequencies in the distribution do conform to the prescribed probabilities, and in the proper ratio of occurrence; that the extremely improbable event, the event with an "infinitesimal probability," does not occur, and may be practically disregarded although the law of chances makes its occurrence necessary some time or other.

But we have already noticed that, since it must occur sometime, it might happen in any series of trials, and in view of the fact that trials of all sorts have been carried out to great numbers and, moreover, that any empirical series of trials may be considered a part of an indefinitely large number of series, it may fairly be asked whether occurrences which, upon the basis of any single empirical series, must be regarded as having an infinitesimal probability, may not be expected to occur, or indeed have not occurred, and consequently could not be safely accepted in the limited empirical series as evidence of an extra-chance cause. This query rises in spite of the admission that the improbability of the event (on the basis of the limited series) is an index to the degree with which it may be disregarded and that scientific certainty is satisfied with a practical limit in which $P = .0000221$.

The event with an infinitesimal probability has not occurred in our empirical series, but it does seem to have occurred elsewhere: *viz.*, in games of chance and in daily life. Let us examine and try to interpret the meaning of some of these instances, and we may as well begin with some events which have been sufficiently striking to be reported in the literature, but the probability of which is far from "infinitesimal:"

E. Desbeaux[195] has communicated three cases in his own experience at Monte Carlo, the detail being reproduced from his diary. His second and third cases follow:

(1) *Tuesday, Dec. 26, 1894.*—M. Desbeaux had to dodge out of the way of a carriage bearing on its lighted lamps the number *22*. At the Casino he placed some money on *22*, but it failed to turn up, and he left. Had dinner and returned. Destiny having failed to furnish him a cue he decided it should be *1*. After he waited an hour *1* turned up; he then put down a *louis* on the *22*. The croupier threw his ball, and *22* turned up. The next day while lunching with friends at the Hotel du Cap Martin, he noticed that the service-card on the table was *222*. At nine o'clock in the evening at the Casino just as he approached a roulette table the *1* turned up.

[195] E. Desbeaux: Trois cas de prémonition. *Annales des Sciences Psychiques,* Paris, 1889, 9:65-70.

Since success followed this cue once before, he staked a *louis* on the *22* again; *22* turned up; he took his winnings and left his *louis* on *22* for a second time, but lost; suspecting that the surplus *2* on the service-card at the hotel was supplied by destiny for his cue, he hurriedly placed a *louis* on *22* a third time, since *2* had just been called. He won. (Two *22*'s out of three).

(2) *February 3, 1899.*—His glance was challenged by the number *11* painted on a wall; he looked about for a cue and at once saw electric car No. *4*. At the Casino while waiting for the *4* to turn up he played *5 franc* pieces for an hour, constantly losing. Upon *4* turning up he staked on *11*; it won. He picked up his winnings and started on his way out, but upon glancing at the table on his left he saw *4* turn up there. He at once put down a *louis* on the *11*, and won again. (Two *11's* in two trials. $P = .000731$).

Other cases of alleged presentiment relating to numbers were communicated to the Society for Psychical Research [196] by Professor G. Hulin of the University of Ghent. It appears that in Belgium young men have often predicted the numbers they were to draw in the conscription for the army:[197]

(3) Five cases of the right numbers being predicted, during the eight years, 1886-1894, are given on good authority; the numbers in the first four cases being respectively 90, 112, 216, 111. The first case was an especially striking one; a clearly externalized vision of the number 90 appeared to the percipient, and produced a strong impression on his mind, convincing him that he would draw that number. In the fifth case, the man who was to draw first announced that his number would be 116, and on being told that that was already drawn, said it would be 115, which turned out correct. The report only professes to give correct predictions, and we have no means of knowing how often predictions of these numbers are made which turn out wrong. Neither are we told how many numbers there were to draw from, except in one case, where it appears that there were at least 150, the lowest of them being 46 and the highest 223. In this case, the number 216 was the one rightly guessed. We must assume, I think, that these facts were known to the man who was to draw, . . . and, if so, the chance of his making a correct guess was, of course, about 1 in 150. (pp. 253-4).

Assuming that the chance of a R guess of a conscription-number was $\frac{1}{150}$ ($p = .00667$), less than 300 guesses of conscription-numbers would need to have been made during the eight years for the five cases to be accounted for by chance.

Richard A. Proctor [198] gives several cases of rare events:

(4) Professor De Morgan, in his 'Budget of Paradoxes' says: "In the French lottery five numbers out of ninety were drawn at a time; any person, in any part

[196] *Proceedings S. P. R.*, 1895, 11:545 ff.

[197] *Idem*, 1899, 14:253-4.

[198] Richard A. Proctor: Chance and Luck; a discussion of the laws of luck, coincidences, wagers, lotteries, and the fallacies of gambling. London, 1899.

of the country, might stake any sum upon any event he pleased, as that 27 should be drawn; that 42 and 81 should be drawn; that 42 and 81 should be drawn and 42 first; and so on up to a *quine determiné*, if he chose, which is betting on five given numbers in a given order." The chance of a successful guess, in this last case, is 1 in 5,274,772,160. ($p = .000,000,000,19$) (p. 144). . . . No instances are on record of a *quine determiné* being won, but a simple *quine*, the odds against which, be it remembered, are nearly 44 millions to 1 ($p = .000,000,022,8$), has been won; and simple *quaternes*, against which the odds are more than half a million to 1 ($p = .000,002$), have often been won. In July 1821 a strange circumstance occurred. A gambler had selected the five numbers 8, 13, 16, 46, and 64, and for the same drawing another had selected the four numbers 8, 16, 46, and 64. The numbers actually drawn were

8 46 16 64 13

so that both gamblers won. Their stakes were small, unfortunately for them and fortunately for the bank, and their actual winnings were only 131,350 francs and 20,852 francs respectively. If each had ventured £1 only, their respective winnings would have been £1,000,000 and £75,000. The coincidence was so remarkable (the antecedent probability against two gamblers winning on a simple drawing or simple *quine* and a simple *quaterne* being about 22 billions to 1 [$p = .000,000,000,045,5$]), that one can understand a suspicion arising that a hint had been given from some one employed at the lottery-office. M. Menut insinuates this, and a recent occurrence at Naples suggests at least the possibility of collusion between gamblers and the drawers of lottery numbers. But in the case above cited the smallness of the stakes warrants the belief that the result was purely accidental. Certainly the gamblers would have staked more had they known what was to be the actual result of the drawing. (pp. 145-6).

Whether these apparently improbable events had an infinitesimal probability would depend upon the number of wagers made. Proctor, from figures given by De Morgan, calculates that about 5,000,000 persons per annum staked money on the simple *quaterne* alone, and quotes Quetelet to the effect that in the five years 1816-1820 the total sums hazarded on all forms of venture in the Paris lottery amounted to 126,944,000 francs.[199] If as many wagers had been placed upon simple *quines* as appear to have been placed upon simple *quaternes*, one might expect a simple *quine* to be won, according to the law of chances, some three times in twenty years.

In another place Proctor reports a remarkable event in the throwing of dice:

(5) Mr. Steinmetz tells us that, in 1813, a Mr. Ogden wagered 1,000 guineas to one that 'seven' would not be thrown with a pair of dice ten successive times. The wager was accepted (though it was egregiously unfair), and strange to say his opponent threw 'seven' *nine times running*. At this point Mr. Ogden offered 470 guineas to be off the bet. But his opponent declined (though the price offered

[199] Proctor: *Op. cit.*, p. 147.

was far beyond the real value of his chance). He cast yet once more, and threw 'nine,' so that Mr. Ogden won his guinea.

Now here we have an instance of a most remarkable series of throws, the like of which has never been recorded before or since. Before those throws had been made, it might have been asserted that the throwing of nine successive 'sevens' with a pair of dice was a circumstance which chance could never bring about, for experience was as much against such an event as it would seem to be against the turning up of a certain number ten successive times at Roulette. Yet experience now shows that the thing is possible; and if we are to limit the action of chance, we must assert that the throwing of 'seven' *ten* times in succession is an event which will never happen. Yet such a conclusion obviously rests on as unstable a basis as the former, of which experience has disposed. (pp. 34-5).

The chance of throwing 'seven' with a pair of dice is $\frac{1}{6}$; of throwing 'seven' nine times in succession is $(\frac{1}{6})^9$, or once in 10,077,696 trials ($p = .000,000,099,5$), which we may regard as an infinitesimal probability when only a short series of throws is considered. The event which did not occur (the throwing of 'seven' ten times running) has a probability of .000,000,016,6. According to the law of chances, as Proctor intimated, either of these events may be expected to occur in a sufficiently great number of trials, and if we were to regard the occurrence of the run of nine 'sevens' as belonging to the aggregate series of all the trials ever made to throw 'seven,' it might after all have a fair degree of probability. Thus, if 11,000,000 trials had been made it was overdue.

Miss Alice Johnson [200] has recorded a remarkable event ensuing from "a guessing competition":

(6) It happens that a good deal of information is available about it, as it was the subject of two trials, the proprietor of the paper having refused to pay the prize money to the successful competitor, who therefore brought an action against him. The jury gave a verdict for the plaintiff, but this was disallowed by the Judge, on the ground that the competition was a "lottery" (that is, a distribution of prizes determined by lot or chance, and involving no skill) and therefore illegal. The judgment was appealed against, and the Court of Appeal determined that the competition was not a lottery. (p. 322).

. . . Mr. Bingham Cox, in copies of the paper (the *Rocket*) of November last, had offered a prize of £1000 to any one who should predict the exact number of male and female births, together with the number of deaths in London for the week ending December 11, 1897. There was also a number of consolation prizes offered for those who might not predict the exact figures, but get very near to them. Hoping to be the £1000 prize-winner, the plaintiff (John Henry Hall, butcher, near Sheffield) bought 252 copies. From these copies he cut out the coupons, filled them up in the required manner, and forwarded them to the offices of the *Rocket*. In one of these coupons, according to counsel, he predicted that the number of births for the week ending December 11 in London would be:—Males, 1,244; females, 1,245; and deaths, 1,866. Plaintiff afterwards obtained from the Registrar-General

[200] Alice Johnson: Coincidences. *Proceedings S. P. R.*, 1899, 14: 322 ff.

or from the Queen's Printers . . . the returns of the male and female births and of deaths for the week in question, and it was alleged that the figures were precisely the same as those that had been forecast by the plaintiff . . . (pp. 322-3).

. . . The point was argued whether the competition was a lottery or not. . . . In a lottery there was, he (counsel for the plaintiff) said, no opportunity given for the employment of any skill or judgment whatever. It depended purely on chance, and no exercise of skill or knowledge entered into it. The present case was entirely different, for the plaintiff had clearly shown that he had taken a considerable amount of trouble to obtain information to enable him to form a correct judgment. (p. 325).

[In the Court of Appeal, the counsel] for the plaintiff contended that this was not a competition of mere chance, but some skill came into it—namely, calculating from previous returns of the Registrar-General how many births and deaths would be likely to take place in London in a particular week. It was not, therefore, a lottery. . . .

Lord Justice A. L. Smith said that, in his opinion, this was not a lottery. The solution of the question did not depend upon mere chance. It depended very largely upon chance, but there was an element of statistical inquiry brought into it . . . Lord Justice Rigby and Lord Justice Collins concurred.

In reply to an inquiry made later in the same month, I was informed that "the *Rocket* and its late proprietor are both dead," so that it seems very doubtful whether the prize money was ever paid. (p. 327).

Although judgment was awarded the plaintiff on the grounds of some element of skill in the competition for the prize, the judge admitted that the solution of the question "depended very largely upon chance," and it is our interest to learn approximately what, after skill is allowed for, the probability of a correct return was. Miss Johnson has estimated this by finding the actual range of the variations in the numbers of male and female births and of deaths in a week, in London, during a large number of weeks, and then calculating the number of possible combinations of numbers within these ranges:

The chance of getting all the three numbers (male and female births, and deaths) right in a single guess is one in 438,770,108. . . .

Since the successful competitor made only 252 guesses, it was extremely unlikely that he should have got one of them right (the chance being one in 1,740,000); while the chance against some one of the total 100,000 guesses said to have been sent in being correct (assuming that the guesses were all different, which they probably were not) was 5,000 to one. (p. 329).

This seems like an event with an infinitesimal probability actually occurring, and if the limit of chance is calculated from this unusual probability by the formulæ customary for the more usual probabilities of a single occurrence, the actual success exceeds the limit of chance some 17 times. Although when the aggregate number of guesses is considered,

the probability of some one guess being right is not entirely negligible, the event itself is striking enough.

Besides the unusual occurrences, like those just enumerated, the improbability of which can be fairly accurately determined, we find in the literature the record of perhaps even more striking events in daily life the infinitesimal probability of which is supposed to be patent. And since these belong to precisely the class of events accepted almost everywhere as *proof* of supernormal or even supernatural causes, it may be worth our while to inspect some of these cases, all of which will probably be immediately granted to be fortuitous; *i. e.*, as depending entirely upon chance.

We may begin the list with the following coincidences which were reported by Miss Johnson:[201]

London, January 21, 1898.

(7) Dr. M. and the undersigned H. W. B. are brothers-in-law. Dr. M. resides and practices in London; H. W. B. lives in a suburb, but is engaged in business in the city. In December, 1897, he was temporarily staying with his brother-in-law Dr. M. The undersigned J. T. H., an Australian on a short visit to London, was detained there by illness. He had not previously known Dr. M. or H. W. B., but in November, 1897, he made the acquaintance of Dr. M., who, on account of the ill-health of J. T. H., was kind enough to invite him to stay at his house. In December, J. T. H. went to stay with Dr. M., and there for the first time met H. W. B.

It transpired in conversation that H. W. B. and J. T. H.

(1) Were both born on May 13, 1858;

(2) Their christening was in each case delayed until the completion of a new church, one church being in England, the other in Australia;

(3) At that time (December, 1897) they each occupied a house (one in England, the other in Australia) which house in each case consisted of two small cottages knocked into one by connecting doorways.

[Signed] Herbert Wilford Brett.
J. T. Hackett.

Mr. Hackett is an Associate S. P.R. He is an old friend of mine, and gave me the above account orally, and afterwards wrote it out at my request.—R. Hodgson. (p. 199).

(8) From a country house, some years ago, we drove over, a large party, to some distant ironworks. During our walk round them we had to stand for a while by a little railway waiting for an engine to get out of the way. Whilst we did so I asked a man of the party who had only arrived the evening before whether he had ever been on an engine. I forget what he said, but I remember expatiating on the fun of driving one—an accomplishment I had lately practiced. He seemed confused, and some elderly ladies who were listening looked shocked, which I fear prompted me to enlarge on the topic. On the way back he confided to me that this was the anniversary of his wife's death. She had died that day three years ago. In the evening I repeated this to our hostess, who then told me the cause of

[201] *Proceedings S. P. R.*, 1899, 14: 193 ff.

her death. It appeared that once indeed the poor man *had* been on an engine and it was with his wife. It had overturned going round a curve, and his wife was crushed to death beneath it. (p. 200).

(9) April 22, 1898.

Dear Sir:—Having been at the meeting this afternoon, at Westminster Town Hall, I write to tell you about a further coincidence in connection with one of the examples recorded by Miss Shuttleworth (No. 8). The gentleman with whom Miss Shuttleworth spoke upon the subject of driving engines was, I feel almost sure, my father. The lady who lost her balance while rounding a corner, and who was killed on the spot, was my mother! It was quite a chance my being at the meeting this afternoon, as I am not a member of the Society, and it will probably be the only meeting I shall be able to attend this year, as I live altogether in Ireland. On coming back here this afternoon I said to my father, "Did you ever meet a Miss Shuttleworth?" and on hearing he had done so, I proceeded to tell him of this afternoon's meeting. He says he remembers having spoken to Miss S. about engines. . . .

April 25, 1898.

. . .It was either in '71 or '72. My mother was on a small traction engine built by her cousins, the present Lord —— and his brothers, and in turning a corner, she put up her hand to steady her hat, and in so doing, she lost her balance and fell off the engine—being killed on the spot.

I thought it odd the other day that this should be alluded to at the one meeting of the year at which I happened to be present, and that the subject of the meeting should be "Coincidences." (p. 202).

Cases 8 and 9 together, as their contents show, are very curious, and recall an experience recorded by Professor Jastrow. He had just stated a generalization to the effect that coincidences in a certain field become remarkably frequent as a result of a strong interest in that direction:[202]

(10) Some years ago I became interested in cases of extreme longevity, particularly of centenarianism, and for some months every conversation seemed to lead to this topic, and every magazine and newspaper offered some new item about old people. Nowadays my interest is transferred to other themes; but the paragrapher continues quite creditably to meet my present wants, and the centenarians have vanished. When I am writing about coincidences, I become keen to observe them; such for example as this: I was reading for the second time an article on "Mental Telegraphy" (by Mark Twain in *Harper's Monthly Magazine,* December, 1891): I was occupied with what is there described as a most wonderful coincidence, the nearly simultaneous origination by the author and by Mr. William H. Wright of a similar literary venture,—when I happened to take my eyes from the page and saw on my desk a visiting-card bearing the name, "W. H. Wright." It was not the same W. H. Wright, but a gentleman whom I had met for the first time a few hours before, and have not seen since. Had I not been especially interested in this article and its subject, the identity of the names would certainly have escaped my attention, and there would have been no coincidence to record. (p. 89).

[202] Jastrow: Fact and Fable in Psychology. Boston, 1900.

The following three cases were quoted by Miss Johnson:[203] the first from *The Spectator* (August 27, 1898); the second from "Over the Tea-cups," by Oliver Wendell Holmes (3d ed.. 1891, pp. 18 ff.); and the third from *Notes and Queries* (1895, 8:270):

(11) Waldron Rectory, Sussex, August 22d.

. . . Many years ago, when at Oxford, my father gave me as a heirloom a ring presented to him by an old friend, and bearing an inscription stating that it contained the hair of the Duke of Wellington. This ring I gave to my wife on our marriage in 1876. In October, 1879, when we were on a visit to Mr. W. Arkwright, of Sutton Scarsdale, my wife felt the ring slip off her finger at the dinner-table, and although careful search was made, nothing more was seen or heard of it for eighteen years, so far as we were concerned.

At the commencement of this year, however, my wife received a letter from her half-sister (Mrs. Hodge) in New Zealand, which stated incidentally that a church in which she was interested out there had received unexpected help some years ago from a curious source. Her sister (Miss White) had sent out from England at her request some gloves purchased at Bide's [a London shop], and on trying on a pair of these gloves she, to her astonishment, found inside one of them a ring containing the hair of the Duke of Wellington, which had evidently been drawn off the finger unconsciously by some one trying on the glove at Bide's. Unable to find the owner of the ring and not liking to keep it, Mrs. Hodge thought it would be a fair thing to sell it and apply the proceeds to the church fund. She did so and the purchaser was a Mr. Frank Arkwright, of Overton, Marston, New Zealand, whose grandmother had given the ring to my father, and who has most kindly replaced it in my possession.

Now, here are a series of coincidences which are only likely to happen once in a life-time. That of all the thousands of people who purchase gloves my wife's half-sister should have lighted upon this particular pair and, unknown to herself and to Bide, should have sent out this ring in them to her sister in the Antipodes, and that there it should have been recovered in the house of a cousin of the Mr. Arkwright in whose house it had been lost eighteen years ago, surely goes to show that sometimes, at any rate, truth is stranger than fiction. As a minor coincidence I may perhaps mention that the letter which, by the merest chance, happened to mention the finding of the ring, was dated from Wellington, in New Zealand. I am, sir, etc., W. J. Humble-Crofts.

(12) One evening while I was living in Charles Street, I received a call from Dr. S., a well-known and highly respected Boston physician, a particular friend of the late Alexander H. Stephens, vice-president of the Southern Confederacy. It was with reference to a work which Mr. Stephens was about to publish that Dr. S. called upon me. After talking that matter over we got conversing on other subjects, among the rest a family relationship existing between us,—not a very near one, but one which I think I had seen mentioned in genealogical accounts. Mary S. (the last name being the same as that of my visitant), it appeared, was the great-great-grandmother of Mrs. Holmes and myself. After cordially recognizing our forgotten relationship, now for the first time called to mind, we parted, my

[203] *Proceedings S.P.R.*, 1899, 14:227-8, 278-9, 226.

guest leaving me for his own home. We had been sitting in my library on the lower floor. On going upstairs where Mrs. Holmes was sitting alone, just as I entered the room she pushed a paper across the table towards me, saying that perhaps it might interest me. It was one of a number of old family papers which she had brought from the house of her mother, recently deceased.

I opened the paper, which was an old-looking document, and found that it was a copy, perhaps made in this century, of the will of that same Mary S., about whom we had been talking downstairs.

If there is such a thing as a purely accidental coincidence, this must be considered an instance of it. All that one can say about it is that it seems very unlikely that such a coincidence should occur, *but it did.*

(13) 49, Edith Road, West Kensington, W.

. . . Christmas, 1892, I spent at Southsea. One day I wanted to get some envelopes of a somewhat unusual size. I tried some dozen stationers' shops in the town, but not one could supply me. I gave up my quest in despair and was returning home to dinner. I had nearly reached the top of Palmerston Road when I saw a small parcel lying on the pavement. I picked it up, and, as it bore no printed address of a shop, I took it back with me to my apartments. You may judge my astonishment when, on opening it, I found it to contain a packet of twenty-five envelopes of the precise size for which I had fruitlessly enquired! My sister, who was with me at the time, can vouch for the truth of this incident.

CHAS. JAS. FÈRET.

The following case has just occurred to the writer:

(14) While preparing the manuscript on this section dealing with Inductive Probability, the writer met with an experience which may be regarded as more or less remarkable. He had filled in the numbers in Fig. 5* and had just finished filling in, in India ink, the alternate compartments down to the *o*, in order to designate what compartments are black, when he was confronted with a serious difficulty. The compartments he understood to alternate black and red; but if *o* is made black, there are two adjacent compartments black, and if made red, there are two adjacent compartments red. He laid down his pen, went home (on the campus) to lunch, and remarked on his dilemma to his wife. She instantly replied: "Why don't you go down into the cellar, look at the Roulette wheel, and find out?" "Roulette wheel! in the cellar?" "I think it must be a Roulette wheel." Sure enough! A strange piece of furniture unearthed in the cellar proved to be a Roulette wheel, made in Chicago, having 28 compartments. Zero is green!

How the Roulette wheel came to be in his cellar, the writer at the risk of his reputation will refrain from explaining, except to intimate that the causes (however improbable in themselves) were neither supernormal nor related to his drawing. He is satisfied with the statement of the problem: What is the probability that an assistant professor of psychology who gets into trouble over the color of a Roulette zero will find in an hour's time, not only within the Puritanic precincts of his university, but in his own cellar, a Roulette wheel that resolves his riddle?

The following brief report of a "double-barrelled" coincidence was

* Shown on p. 328, *supra.*

sent to the writer by Dr. W. W. Campbell,[204] director of the Lick Observatory:

(15) Perrine of our staff discovered his first comet, and in due time it disappeared behind the sun. A few weeks later a German astronomer re-observed it (we had already been observing it again for fifteen days) and sent us a cablegram announcing the position of Perrine's first comet. The cablegram came correctly, but our astronomer who received it made a mistake in translating degrees and minutes of right ascension into hours and minutes of right ascension, and the erroneous position was handed to Perrine. Perrine looked at the erroneous position the next morning just before daylight and there was a comet in the field of view. This turned out to be a new comet, one that nobody had ever seen before. The new comet was traveling on the face of the sky at high speed, and if Perrine had looked at that point on the morning before or the morning after, or any other morning, it would have been far outside the field of view and might have come and gone totally unseen. It was a sort of double-barrelled coincidence.[205]

The chance of a miscalculation locating a point in the path of a comet, and an unknown one, is certainly very small; and the chance that the swiftly-moving comet should occupy that position at the moment Astronomer Perrine made his observation is certainly infinitesimal.

Another coincidence to occur to an astronomer, which for sheer downright mischance can scarcely be excelled, has been quoted from De Morgan by Proctor:[206]

(16) An old woman came to Flamsteed, the first Astronomer-Royal, to ask him whereabouts a certain bundle of linen might be, which she had lost. Flamsteed determined to show the folly of that belief in astrology which had led her to Greenwich Observatory (under some misapprehension as to the duties of an Astronomer-Royal). He "drew a circle, put a square in it, and gravely pointed out

[204] To whom the writer hereby acknowledges his obligations.

[205] A more detailed report will be found in *Science*, 1917, 46:36-37, which had not yet been issued at the time the above text was written. A minor coincidence in connection with Comet *a* 1896 is also worth noting, since it points to the dependence of the frequency of coincidence upon interest in this direction to which Jastrow has called attention. This Comet was discovered in February, 1896; the coincidence involved was reported in *The Observatory* in 1903 (26:293-4) and not likely to become generally known; I first heard of it at a dinner to psychologists, April 7, 1917. Early in July I was working on the text on coincidences and wrote to Dr. Campbell on July 8th asking for details and begging him to publish them in some general scientific journal. In his reply Dr. Campbell summarized the story, and said that three or four weeks earlier he had read proof on the article which later appeared in the July 13th number of *Science*. That my need for the coincidence should occur when the article prepared for the public was in type for the first time after a delay of twenty-one years is remarkable; and it is no less remarkable that my appeal to Dr. Campbell to publish his story should reach him after he had read proof on it and just before it appeared.

[206] Proctor: Chance and Luck. 1889, p. 195.

a ditch, near her cottage, in which he said it would be found." He then waited until she should come back disappointed, and in a fit frame of mind to receive the rebuke he intended for her; but "she came back in great delight, with the bundle in her hand, found in the very place."

The following case was quoted by Tuckett from the (London) *Daily Graphic* (September 7, 1905):[207]

(17) Sir:—Among many strange coincidences which I have experienced in my time, one of the most singular which I can recall at the moment happened to me in connection with a play which I wrote some twenty years ago for the German Reed entertainment. One of my characters was named Robert Golding, and for the requirements of the plot I had made him the sole survivor of the crew of a ship called the Caroline, which had been lost at sea. A few days after the production of the play I read in a newspaper an account of the shipwreck of a vessel named the Caroline, which had gone down with all hands, with one exception, and this exception was a man of the name of Golding. Now Golding is not at all a common name, and the circumstance of his being, both in fact and fiction, the sole survivor of the shipwrecked Caroline, impressed me at the time as being a coincidence of a very peculiar nature. Yours faithfully, ARTHUR LAW.

Is the nature of the following coincidence, offered by Mr. Beckles Willson[208] as evidence for thought-transference, any more peculiar?

(18) A prominent Chicago journalist, Mr. F. B. Wilkie, reported that his wife asked him one morning in October, 1885, while still engaged in dressing, and before either of them had left their sleeping-room, if he knew anyone named Edsale or Esdale. A negative reply was given, and then a "Why do you ask?" She replied: "During the night I dreamt that I was on the lake-shore and found a coffin there, with the name of Edsale or Esdale on it, and I am confident that someone of that name has recently been drowned there." On opening his morning paper, the first item that attracted his attention was the report of the mysterious disappearance from his house in Hyde Park of a young man named Esdale. A few days afterwards the body of a young man was found on the lake-shore.

Or is this case, which was reported by Bertram W. B. Greene?[209]

(19) Paris, December 4, 1895.

In August, 1889, Mrs. S. was staying at Newcastle, New Hampshire, U. S. A. One night she dreamed that she received a letter, in an unknown handwriting, stating that she had been left a bequest of $5,000, but that she would not receive it immediately, owing to certain legal formalities. Mrs. S. remembers that the letter caused her pain in her dream, as the only person likely to leave her money was one of her brothers, of whom she was very fond. The dream was very vivid.

On waking, her breakfast was brought into her room, while Mr. S. went downstairs to eat his. A short time after, he returned with a letter, the envelope

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[207] Tuckett: The Evidence for the Supernatural. London, 1911, p. 120.

[208] Willson: Occultism and Common-Sense. London, 1908, p. 65.

[209] *Journal S. P. R.*, 1896, 7: 257-8.

of which was addressed in Mrs. S.'s sister's handwriting. On opening the envelope, two sheets fell out. One of them Mrs. S. immediately recognized as coming from her sister; the other was in an unknown handwriting, and Mrs. S. picked it up and looked over it without unfolding it. Suddenly she caught sight of the figures $5,000 in the text of the letter. She let it fall with the exclamation: "Why, I dreamed that!" Mr. S. states that the expression of her face was one of extreme astonishment.

On reading the letter, it was found that an uncle of Mrs. S., who had died six months before, had requested that she should be given $5,000. She never expected anything from him at his death, as he had four children. *The money could not be given her for some time, owing to legal formalities.* She had not been informed of the request before, owing to the aforesaid formalities.

BERTRAM W. B. GREENE.

These last two cases are inserted as typical evidence of supernormal causes, for the purpose of showing their similarity to the preceding coincidences, and their consequent unreliability as indications of anything beyond chance. Sometimes this fact is sufficiently realized by students of coincidences to lead them to generalize it so that it also covers the cases in which the emotional element associated with consanguinity is the typical index of the supernormal nature of the phenomenon:[210]

(20) In another dream I found myself, without surprise, seated in a Protestant church, notwithstanding that I had some years before become a Catholic, but I thought I was greatly concerned in keeping the place next me for a friend who was to join me there. To my dismay, a gentleman came, "without with your leave, or by your leave," and established himself in this place in a peremptory manner. I turned round with the intention of evicting the intruder, though my hints had no effect upon him, but in doing so I observed that he was Canon Kingsley. The next morning the first event that caught my eye in the newspaper was the announcement of Canon Kingsley's death. Now, I knew no more of Canon Kingsley than of the Shah of Persia; I had seen both in public, and I had read a book by each, and there my acquaintance ended. I had not been thinking or talking about him. It could only have been a kaleidoscopic mixing up of images in the brain—yet, had it been some particularly dear friend whom I had thought I felt placing himself so unexpectedly by my side, and had that friend also died unknown to me the day before, it would have been said by all ghost-believing people that it was the actual spirit of the dear departed. As this was certainly no apparition of the sort, I argue that in the cases where the condition of affection enters into the details of the case, they are yet nothing more than fortuitous coincidences either.

But perhaps the most overpowering character of a coincidence is the inclusion of a number of coincidental factors. It may be recalled that Mark Twain, concerning the coincidence alluded to in Case 10 above,[211] said:[212]

[210] *Journal S. P. R.*, 1885, 1:230.

[211] *Supra*, p. 354.

[212] *Harper's Monthly Magazine*, December 1891, 84:97.

Necessarily this could not come by accident, such elaborate accidents cannot happen. Chance might have duplicated one or two of the details but she would have broken down on the rest. I cannot doubt—there is no tenable reason for doubting—that Mr. Wright's mind and mine had been in close and crystal-clear communication with each other across 3000 miles of mountain and desert on the morning of the 2d of March.

In support of a widely operative "Mental Telegraphy" he instances the independent simultaneous inventions and literary creations; such as, the telegraph by (1) Henry, (2) Wheatstone, (3) Morse, and (4) a German in Munich; the theory of evolution by Darwin and Wallace; Miss Alcott's novel, "Moods," which had to be extensively revised because its plot and its leading characters (even to names) were too similar to Miss Crane's "Emily Chester," which was in press; Voltaire's "Candide" and Johnson's "Rasselas"; and then queries:

Is it not possible that inventors are constantly and unwittingly stealing each other's ideas whilst they stand thousands of miles asunder? (p. 98).

The primary question to settle is whether this particular extra-chance cause is operative. Until that fact is established to the satisfaction of science, the possibility of its operation is a remote consideration. Now, of course, the only method of solving the question is by tabulation of coincidences; and if reliance is placed upon cases with an infinitesimal probability we must be sure, in the first place, that chance is not the sole cause, and, in the second place, that all other extra-chance causes are eliminated. Neither of these requirements is met in Mark Twain's "elaborate" coincidence.

Before bringing together our argument on the fallacy of the infinitesimal probability, let us examine an elaborate coincidence which is to be attributed to chance causes alone, and then consider some of the extra-chance causes which cannot be eliminated from the type of cases most commonly accepted as evidence for supernormal causes.

The following "elaborate" coincidence was communicated some thirty-eight years ago by Dr. George M. Beard:[213]

(21) The first letter is a so-called "April-fool" letter, as the date suggests, and is wholly imaginative. It was written for amusement purely, and obtained a very different reply from what was expected.

The author of the communication is a well-known merchant of this city, and a friend of mine. The person who replied is also well known in the region where he resides.

This coincidence is certainly one of the most remarkable of any recorded in the history either of logic or of delusions.

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[213] George M. Beard: A remarkable coincidence. *The Popular Science Monthly*, 1879, 15:628 ff.

202 Columbia Heights, Brooklyn, April 1, 1879.

My Dear Sister Velina: You will no doubt be somewhat surprised to receive a letter from me, but I have a little matter of business, and if you will attend to it you will place me under obligations to your good self.

Some time ago a man by the name of John Nasium lived in New York. His father was a Southerner, and died last summer of yellow fever. . . .

This John Nasium seems to have been the black sheep of the family, and when he left New York he did not leave a very good record behind him. He went from here to Toledo, Ohio, and afterward, we hear, he went to Tecumseh, Michigan, no doubt thinking that in a quiet country place he would be more secluded than he could be in a city. I and several of my friends would like to get track of him, if it can be done quietly, and without exciting any suspicion. He may have changed his name, and so I will describe the man, as nearly as I can, which may be some help to you. . . He is rather tall, weighing about 180 pounds, I should think. He stoops a little, and is slightly lame in the left leg. You would not observe his lameness, unless you were to pay particular attention to him while walking. His hair is a dark sandy color, in fact almost a red, and his side-whiskers are almost the same color, but a little darker. He is about thirty-eight years of age, but really does not look over thirty. His eyes are a very dark brown, and the left eye looks a little peculiar, *i. e.*, unlike the other—looks as if some time or another a cataract had been removed by an operation. To look at him, you would at once see a difference in his eyes, and yet I cannot describe the difference any better than I have done. While he lived here he usually wore his hair rather long, and carried himself in a style peculiar to the Southerner.

Now, perhaps the best and most prudent way for you to do would be for you to go up and read this letter to Uncle Hiram first. He is a very careful, discreet man, and he can make inquiries and excite less suspicion than you could.

I am real sorry to make you any trouble, and much less Uncle Hiram, but this is a matter, if it can be properly done, which may be of considerable importance to me and several of my friends, and perhaps further the ends of justice.

There is one other mark which may aid you, which is—this man was in the rebel army, and *his forefinger on his left hand was shot off*. His nose is quite prominent, and he has a very mild and quiet look, and he is the last man you would pick out for the scoundrel he is. Yours very truly,

R. T. Bush.

P. S.—Please attend to it, and oblige.

Shortly after this letter reached its destination, Tecumseh, Mr. Bush received a telegram stating that the man had been found, and asking if they should arrest him. The correspondent had not observed the date of the letter, nor did he suspect that he was reading a novel; and in a few days the following letter was received:

Tecumseh, April 18, 1879.

Mr. R. T. Bush—

Dear Sir: Velina read to me a letter Wednesday evening from you, describing a certain man that was wanted in New York, who had recently left Toledo for this village.

The next morning, after hearing the description, I informed our marshal of the fact, and requested him to keep a lookout for such a man. In the course of

half an hour he came to me, saying that he had just seen my man—with sandy whiskers, rather tall—would weigh 170 or 180 pounds—wearing specs, and the front finger of the left hand missing; and was very anxious that he should be immediately arrested, as he was then at the livery-stable, for a saddle-horse to ride away. I told him we had better wait and be sure that he was the one we wanted, and also find out if we could whether you wanted him arrested, should he prove to be the right man. I saw the man, and he answered the description so well, even to the *finger,* that I thought best to telegraph you for instructions. The marshal, in the meantime, was to keep his eye on him (as he failed to get a horse). Seeing him walk down to dinner with one of our townsmen, the first opportunity he made some inquiries of this townsman, and found that he was not the man—that he was the cousin of this man that took him to dinner, and was brother to a Mrs. Palmer, whom he was visiting—that he lives in South Cleveland, Ohio, and is a lawyer by profession.

That he answered the description, both in size and the loss of the finger, as well as the color of his whiskers, there could be no doubt. Wearing specs we supposed was to hide the defects of that eye you mentioned, and he looked as though his side-whiskers had recently been cut or shaved; but if, as we were told, his home is in Cleveland, and his name is Hick, why of course we were deceived in the matter. And, if his friend has not informed him, he is still ignorant of our suspicions.

Now, as this is my first experience in the detective business, you will pardon the blunder.

Hoping that it has put you to no inconvenience, I remain yours, etc.,

H. Raymond.

The one striking feature of this coincidence is of course the *loss of the forefinger* in the *left* hand.

Both the imagined and the real case possessed this very exceptional peculiarity. This is a subject on which statistics can not be gained; but it is certain that in the whole continent not a small roomful could be found possessing precisely this deformity at the age specified; and it may well be doubted whether in the whole world there is another person thus mutilated and at the same time possessing all the general physical characteristics of the individual described in the letter.

More striking still is the fact that this individual did not reside in the place where the letter was sent (which is not a large place), and was there by chance only the day that the letter reached there.

Those who believe that the mathematical doctrine of chances can solve the complex problems of coincidences will find in this case material for consideration. . . . It is to be recognized that coincidences of the most extraordinary character and astonishing nature are liable to occur at any instant, and that they are as likely to occur on the first trial as on the last of a long series.

A second point of great psychological interest in this case is the attempt made by the person to whom the letter was addressed to overlook certain discrepancies between the imaginary and real individual, and to twist and pervert and reason upon the facts of the case, so as to bring them into harmony with what he was *expecting to see.* While the man corresponded to the description in size, in the color of his whiskers, and especially in the loss of his finger, he did not correspond in the fact that he wore spectacles and had no side-whiskers. The detective

reasoned that he wore spectacles to hide the defect in the eye, which defect he did not see; and he assumed, on thought, that the side-whiskers had been recently shaved or cut. Nothing is said of his stooping, or of his being lame in the left leg, or of the color of his hair, or of its length.

The bearings of this whole history on the delusions of clairvoyance and mind-reading are apparent. . . . A successful coincidence of this kind would have made fortune and favor for any clairvoyant or mind-reader.

From this exhibit of coincidences, which could be indefinitely extended, one might reasonably infer that chance is not only capable of providing, but is constantly providing, coincidences which have an infinitesimal probability, and his inference would be in accord with the law of chances which provides for the occurrence of such events, in any given series of indefinite length, some time or other. *Within our field of observation there is an indefinite number of series of indefinite length in constant process; the infinitesimally probable events in the aggregate of these various series may be expected to occur frequently.* "The improbable is to be expected in its proper proportion." Consequently, as long as the series in which the event is expected to occur is not specified, or the probability of occurrence of the event cannot be calculated, nothing can be inferred from the frequency of such events.

Let the series be designated, and if the frequency of occurrence is beyond the limit of chance deviation, some evidence for an extra-chance cause is adduced. But many other causes, besides telepathy or lucidity or communication from excarnate intelligences, are known to be present, in the most typical illustrations of supernormal causes, and unless they are excluded the evidence is inconclusive.

Common associations, common characteristics in mental processes (such as inference), common interests, and common desires and ideals, are all potent factors working for startling coincidences. Had Mark Twain's "remarkable" coincidence been even more remarkable than purely chance coincidences which have occurred, these causes would yet stand in the way of his precipitate interpretation in favor of thought-transference; and the same may be said of most of the coincidences recorded as evidence for supernormal causes.

Let us examine again a case at hand which illustrates how the force of common associations of ideas among strangers may raise an event from an infinitesimal to a high probability.

If three given ideas (expressed by single words) have occurred to the minds of 160 persons, what is the chance that there will be a coincidence in the 4th, 5th, or 6th ideas which occur in these 160 respective minds? On the assumption that there are only 160,000 available ideas, $p = \frac{1}{160,000} = .000,006,25$,—*an infinitesimal probability.* And if in each of

the three aggregates of ideas 160 ideas are expressed, the probability of a coincidence by chance remains the same for each aggregate. We should expect a coincidence in 1000 aggregates. In our word-association experiment,[214] in which these conditions were fulfilled, there were 93 coincidences on one word in the first aggregate, which shows in that particular case a probability of $^{93}/_{160} = .58$; and 18 on another word, making on two words alone, $p = .69$. Similarly, neglecting all coincidences except those made on the two most frequent words, for the second aggregate $p = .29$; and for the third aggregate $p = .19$. Or, if we consider the number of separate responses in each aggregate, we should expect 160 separate and distinct ideas in each aggregate if chance alone was operative in determining the responses, the chance for coincidence being $p = (160–160)/160 = 0$. But the total number of words used in the aggregates we found to be not 160, but 35, 68, and 89, respectively, making the respective chances for coincidence, $p = .78$, $p = .58$, $p = .44$,—*very substantial probabilities.*

Coincidences in daily life effected by common mental processes could be recounted in numbers. Here are but two taken at random; the first from Royce's "Report on Phantasms and Presentiments," [215] the second from an article by F. W. H. Myers:[216]

(1) On the day of Mr. Lincoln's address at Gettysburg, I was walking at about the time when, as I supposed, the exercises were taking place. Remembering this I tried to invent such a speech as Mr. Lincoln, or I in his place, would probably deliver. I was astonished the next morning to find that I had duplicated his address, from the third or fourth sentence to the end; and to the passage "It is for us the living rather to be dedicated" I had given almost exactly the words.

(2) While this paper is passing through the press I have received Hellenbach's just published "Geburt und Tod" (Vienna, 1885), in which conclusions much resembling these are advocated, with some singular, even verbal, coincidences with an article on "Automatic Writing" which I published in the *Contemporary Review* for February last, and which Herr Hellenbach cannot possibly have seen. That two persons should independently hit on so *bizarre* a metaphor as "a blue and a yellow consciousness," might seem an impossible chance; but see *Contemporary Review,* 1885, p. 234; and "Geburt und Tod," p. 66.

All these psychological causes for coincidence are, of course, greatly magnified among friends and relatives, because of common training, common experiences, common interests, and common hopes and desires.

The influence of these causes upon coincidences that have an infinitesimal probability is but a special case of their influence upon judgment, already considered under the head of "Explanatory Considera-

[214] *Supra,* p. 307.

[215] *Proceedings Am. S. P. R.,* 1889, Series I, 1:373.

[216] *Proceedings S. P. R.,* 1885, 3:27, footnote.

tions" [217] and "Application of Mental Habit to Experiments in Thought-Transference," [218] and it is sufficient to our present purpose if we insist that here (where the probability of an event is infinitesimal) as elsewhere it must be eliminated from the data which receive statistical treatment in search for supernormal causes.

It is obvious, too, but it cannot be too clearly realized, that the data subjected to statistical treatment must be free from all the errors resulting from the fallibility of human testimony, including mal-observation [219] and the tricks of memory [220] on the part of the original observer. Coincidences in daily life, such as have been enumerated above (from No. 7 to No. 21) and their analogues in the field of psychical phenomena, are not adapted to statistical enquiry, partly for this reason, and partly for the reason stated in the preceding paragraph, but principally because the probability of occurrence cannot be calculated and, consequently, no one can tell how frequently they ought to occur by chance.

The fallacies underlying a reliance upon events with an infinitesimal probability as evidences of supernormal causes therefore are as follows:

1. The events most frequently cited are striking coincidences in daily life subject to the errors due to the fallibility of human testimony, and to the operation of normal extra-chance causes; their theoretical expectation cannot be calculated, and their frequency cannot be known to exceed the frequency prescribed by the law of chances. Owing to the errors which they include, their frequency may be expected to be high.

2. When the event permits a fair estimation of the probability of occurrence, but may belong to any one of many classes of events, the frequency of its occurrence is likely to be regarded in relation to its own undesignated series (when its probability is infinitesimal) instead of in relation to the many classes of events comprehended within the field of observation (when its probability is appreciable). These events are to some degree comparable to Cases No. 4 and No. 6, above.

3. Since an extra-chance cause is indicated by a definite deviation of the empirical frequency from the theoretical frequency, the limitation in frequency imposed by the infinitesimal probability restricts the numbers in the pertinent ratio to such a degree that the ratio is unnecessarily unreliable.[221]

The disabilities of the procedure with an infinitesimal probability are removed by adopting a more favorable procedure in which the following conditions are met:

1. The events subjected to statistical treatment are not contributed by testimony, but are observations recorded by a competent observer, as scientific method demands, on the spot.

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[217] *Supra*, pp. 281 ff.

[218] *Supra*, pp. 291 ff.

[219] *Cf.*, *Proceedings S. P. R.*, 1886-7, 4:381-495; 1892, 8:253-310.

[220] *Cf.*, The Judge Hornby Case: *Nineteenth Century*, 16:89-91, 851-2.

[221] *Cf.*, *supra*, pp. 332-3.

2. The events are not "selected." They are all of the events of their class that were observed to occur, and their observation was not directed by interest in only a part of their class.

3. The probability of occurrence of a single event in the class is known *a priori*. It is not small,—never infinitesimal; preferably less than .5 and more than .025, or within these limits, including both, as in the guessing of the color, and of the complete card, when a set of 40 playing cards (court cards eliminated) is used.

4. The number of trials in a set inspected for R cases might vary from 10 to 1000 or more; the smaller set being preferable when a "distribution" of R cases is to be inspected.

5. The same results should be inspected in sets varying in size, for per cent of R cases, and for a distribution of the R cases in the various sets of the same size. The formulæ given in PART I are applicable for testing for an extra-chance cause.

It may be seen that the experiments in PART I meet these requirements, and perhaps the best conditions are afforded in the guessing of playing-cards. If the R guesses on the individual card, on the color, on the number of spots, and on the suit, are tabulated separately, the probability varies as follows: .025, .5, .1, .25; and should an extra-chance cause be found its association with color or number or form could be tested. If "control" experiments are provided, as they were in our experiments, in such a way that the reagent or percipient can not distinguish a "control" experiment from a "regular" experiment, and an extra-chance cause is found, its dependence upon the knowledge in the experimenter's mind could be determined. The further variation in the conditions, as in our card experiments, would permit an immediate psychological analysis competent to throw light upon the nature of any extra-chance cause that should be found.

The card experiment, as described in PART I,[222] is commended as a most suitable means of acquiring data which, if treated with the formulæ derived from the theory of probability, must certainly resolve the controversy and doubt concerning the alleged phenomena of thought-transference, lucidity (clairvoyance), or the communication of discarnate personalities capable of knowing facts in our world.

[222] The Guessing of Playing-Cards, pp. 48 ff., *supra*.

PART IV.

EXPERIMENTS IN SOUND ASSIMILATION

There he could sit receiving orders—as it seemed to him—to pick up a hymn-book, find out the hymn and take part in the singing verse by verse while the invisible choir were singing most heavenly above him. It was especially the fine strong hymns which were sung to him while he in his book followed up every word with his finger.—BJORNSON in "Wise-Knut," pp. 29-30.

In the ordinary hearing of speech half the words we seem to hear are supplied out of our own head.—JAMES, in "The Principles of Psychology," vol. II, p. 323.

PART IV.
SOUND ASSIMILATION.

INTRODUCTION.

To those who wish to eliminate all sources of error from the proof of the alleged supernormal phenomena, such as the subliminal tapping of thoughts or forgotten memories of a "sitter" at a 'séance', as the tapping of a cosmic reservoir of knowledge, or as the reception of verbal communications from discarnate personalities, there is a highly important consideration, long known to experimental psychologists, but nowhere, so far as the writer knows, subjected to rigorous and straightforward investigation, which involves a deception of the ear in apprehending vocal language. It is an illusion resulting from the psychological process of 'assimilation' to which perception through each of the special senses is subject; and since it applies in this case to the perception of vocal language we may call the general phenomenon into which we wish to inquire "the assimilation of sound in the perception of English speech," or, briefly, "Sound Assimilation."

This error might also be included under the broad term of "Mal-observation," a matter of serious discussion and of experimental investigation in psychical research, especially since the observations of Professor and Mrs. Sidgwick,[1] the report by Professor William James,[2] and the classical experiments with Mr. S. J. Davey conducted by Dr. Richard Hodgson.[3] In the work already done, much has been learned concerning the possibility of erroneous inferences from the sensations of sight and touch, especially in the dark séance where they commonly receive stimuli of only minimal intensity. But the possibility of erroneous inferences from minimal auditory stimuli, especially when these stimuli purport to be language, has been almost completely overlooked, in spite of the

[1] *Proceedings S. P. R.*, 1886, 4:62-74, 99, 103; 1889, 6:4, 5.

[2] Report of the Committee on Mediumistic Phenomena. *Proceedings Am. S. P. R.*, 1887, Series I, 1:230-6.

[3] Hodgson: The possibilities of mal-observation and lapse of memory from a practical point of view. *Proceedings S. P. R.*, 1887, 4:381-495; 1892, 8:253-310.

fact that much striking evidence for the alleged supernormal phenomena mentioned above rests upon the testimony of observers who were exposed to this type of error. A couple of illustrations will make this clear, and in order to avoid seeming to impeach any particular testimony (which of course *may* be free from the error), we shall make them hypothetical:

(1) A sitter in a séance where discarnate personalities purport to speak through a 'trumpet' or in an 'independent' voice, will often hear the names announced by the speaker in so weak and indefinite a voice or whisper, that they are indistinguishable. To a given sitter, however, the sound suggests a familiar name, that of a late friend or relative. It may be a very unusual name. The suggestion is so strong and has come so unexpectedly that the name is recognized, and the veritable presence of the discarnate friend is confidently attested. Other names of mutual friends may follow in the same way and meet a like recognition, effecting so strong a semblance of free communication with a friend that the sitter is overwhelmed with the sense of the super-normal. There was no "pumping" on the part of the medium; there "was no guessing"; the sitter "gave no information"; he was a complete stranger to the medium, who could not therefore "consult a blue-book"; yet his unseen communicant knew every member of his circle of discarnate and incarnate friends! Supposing the sitter was not in a highly emotional state, and did not have a strong expectation of any particular names before the names were recognized, to what extent was error in perception possible? Is he safe in assuming that he can "believe his ears"?

(2) A discarnate personality purporting to be speaking through the 'medium' is often identified by a phrase, a sentence, an injunction, a quotation, or a bit of song, which the sitter knows to be characteristic of the personality purporting to be present. What risk does the sitter take in recognizing the words? Often the language is in a tongue unknown to the medium but known to the sitter. And after communication through a tongue unknown to the medium is accepted as a fact, speech in a tongue unknown to anyone present is often accepted as evidence of the 'control' of some discarnate personality to whom that tongue was native.[4] Assuming, again, that the sitter is not in a highly emotional state, and is not

[4] *Cf.,* Speaking with Tongues (Podmore: Modern Spiritualism. London, 1902, vol. I, pp. 257 ff.); also, the Martian language of Mlle. Helène "Smith" (Flournoy: From India to the Planet Mars. New York, c 1900, pp. 195 ff.) and of Mrs. "Smead" (Hyslop: Apparent subsconscious fabrication. *Journal of Abnormal Psychology,* 1896, 1:202-6); and the Egyptian language of Mr. "Le Baron" (William James: A case of psychic automatism, including "speaking with tongues." *Proceedings S. P. R.,* 1897, 12:277-297).

keenly expectant of any particular phrase, to what extent is the perception of a phrase or sentence in English unreliable?[5]

This enquiry is not so theoretical or academic as it might at first glance seem, for it grew out of a practical effort to put the evidence for a case of glossolalia (the gift of tongues) in a form that would satisfy the demands of science. A man, who is a devout spiritualist, highly intelligent and with a practical turn of mind, moved with his family from a city about a thousand miles away to a city close to Stanford University in order to have his wife's gift examined in a scientific way. Both were convinced that "messages" in both speech and song in foreign languages had been delivered through her mediumship. She is intelligent, and of a character of the highest moral type, but she knows only English. They exhibited affidavits, sworn to before duly authorized Notary Publics, attesting specific instances at specified times and places. One affidavit attests that the medium sang without accompaniment from the platform a sacred song in Italian; another that she sang in the Gottland Island language (a dialect of Swedish) a sailor's song composed by Anna Johnson, the first line of which is "Dit ingen syster går."

They realized that the testimony rested upon the capacity of persons who might not be regarded as competent to make scientific observations, and they proposed having some songs and discourses in unknown tongues recorded upon a dictaphone, which they purchased for the purpose, in order that linguists might examine the records with care. This was done, and some of the best records were examined by linguists, but without identification of language.[6]

This, then, is our immediate problem: *Given the English language spoken under conditions adequate for communication, to what extent does perception of words depend upon the sense of hearing, to what extent upon what the mind supplies?*

Work on this problem has been carried on during four school years: (1) With the assistance of 107 reagents, 40,500 experiments have been performed with "words" or nonsense-syllables. The following table

[5] The identification of a phrase in a tongue foreign to the medium, but known to the sitter, or a phrase in song would, of course, not be so reliable as the identification of a phrase spoken in a tongue native to both of them.

[6] Records have since been made which contain the unknown tongue, the name of the personality purporting to be the speaker, or vocalist, and his translation into English. But so far, the nationality, and the period of the personality have not been given, and none of the tongues has been checked up by competent authority. As soon as the language can be named by a personality who speaks a known tongue, the records will be examined by experts in that tongue. A further problem arises, of course, as to the adequacy of the dictaphone for this purpose.

gives the number of reagents, the number of sets of "words" recorded, and the number of experiments, for the respective years:

Year	Reagents			Sets	Experiments
	Women	Men	Total		
1913-14	18	20	38	123	10,100
1914-15	6	6	12	54	4,100
1915-16	12	15	27	127	12,700
1916-17	20	10	30	136	13,600
Totals	56	51	107	440	40,500

And (2) during the last two years, with the assistance of 32 reagents many experiments have been performed with "simulated English text" on dictaphonic records.

DIVISION I.—NONSENSE-SYLLABLES.

METHOD.

The telephone is now in wide use as a means of communication, and its reliability is attested by the importance of the transactions (business, social, political, etc.) which its patrons commit to it; and the dictaphone is also in general use, from which typists take the dictation of business correspondence directly upon the typewriter. These are the conditions which we chose as "adequate for communication." To them we added several others: (*a*) the perception of speech enunciated clearly in a conversational tone at a point 25 meters distant from the speaker, the clear air in a long quiet room intervening; (*b*) the perception of speech through a partition, or a closed door, and (*c*) directly across a table in a small room.

Now, if the communication—the perception of the spoken English words—commonly depends upon hearing all of the sounds spoken, or if the recipient of the communication *can* hear all of the sounds spoken, the same sounds could be recorded without error when they are pronounced under the same conditions, but in a haphazard order (the recorder being given all the time he needs for recording each syllable).

For material to use in these communications of 'scrambled' English sounds, lists of syllables were compiled, the following first ten words of which will show their nature:

1	chain	chack	pav	shug	pu	uv	su	ur
2	tin	kaf	tez	duf	tu	uz	ku	uth
3	jan	choom	cheen	gub	chu	un	ru	ut
4	sit	lib	kire	chud	ku	ur	ju	uf
5	nap	pooz	fabe	thul	fu	ub	su	uv
6	stawv	hoongash	soje	tus	su	uj	du	uk
7	sprain	reeg	lig	vutch	lu	ug	mu	us
8	voze	keeliv	med	kun	mu	ud	shu	uk
9	claim	koong	wung	sut	wu	ung	bu	ur
10	kerr	chith	yajd	ruj	yu	ujd	chu	uj

Each list (except the second)[7] is composed of 100 syllables, and dif-

[7] This list is composed of 50 syllables or compounds of syllables, and was compiled at the Museum of Anthropology, Affiliated Colleges (University of Cali-

fers in some essential respect from each other. The "chain" list contains 58 words, the others are composed of nonsense-syllables. In the "pav" and "shug" lists, each syllable begins and ends with a consonantal sound; in the former, the intermediate (vowel) sound is varied, in the latter, it is uniformly the indefinite sound of "short u" (ŭ). The "pu" and "uv" lists are made up from the "pav" list by separating the consonant sounds and supplying the "short u" sound; the consonants of the former list are all initial, of the latter, all final, as they are respectively in the "su" and "ur" lists. In the "chain" and "chack" lists the frequency of occurrence of the initial and final (and in the latter of the medial also) consonantal sounds is very irregular; in the "pav," "pu," and "uv" lists the frequency of the respective initial and final consonantal sounds is quite regular, but the simple sounds occur more often than the coalescent sounds (such as *pr, spl, lt, nch*); in the "shug," "su," and "ur" lists, the frequency of the consonant sounds is uniform. And in the "pav," "pu," and "uv" lists about all of the common simple and coalescent initial and final consonant sounds used in English are included, while in the "shug", "su", and "ur" lists only the simple consonant sounds are included.[8]

The general procedure was as follows: The students were assigned work in pairs; each member of a pair recorded from the dictation of the other.[9] Usually each student as experimenter dictated from his own list, and as reagent recorded from a different list dictated by his partner,[10] each reagent having to depend wholly upon his ear for acquaintance with the syllables he was recording. Each pair commonly recorded under at least three general conditions, (1) from the dictaphone, (2) from the telephone, and (3) through 25 meters of air-space, and in the order indicated, although during the first two years some variations were tried in order to cancel a supposed influence of practice.

Before the recording of each list was begun the adequacy of the conditions for communication was tested by communicating and finding them to be satisfactory (this applies particularly to the 25-meter air-space, and

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fornia) San Francisco, for the purpose of testing the dictaphonic record as a means of preserving Indian languages. I am indebted for this list to Dr. A. L. Kroeber, to whom I hereby express my appreciation.

[8] The "pav," "shug," "su" and "ur" lists are given in Appendix A.

[9] This permitted a wide range of variability in the performance resulting from the various combinations of students who spoke and who heard with varying degrees of efficiency. Their results, consequently, might be expected to be applicable to the general conditions of the perception of language.

[10] One variation from this procedure must be noted. The lists on the dictaphones were all carefully dictated by the writer.

to partitions and closed doors; the dictaphones and telephones were used in the customary way and were uniformly found by test to be satisfactory). The dictaphones used were of the customary commercial type, one an Edison model, the other the Columbia, both driven by the electric light current. The telephones were also the customary commercial instruments, the dictating being done at the wall telephone in the Psychology Library and the recording at the desk telephone in the Division of Psychical Research, the connecting being done at the University central.

The following table shows the number of the various lists recorded under the various conditions of communication, and the number of experiments performed with each list, and under each of the conditions.[11]

TABLE XCIII.
Number of Lists and of Experiments.

	chain	chack	pav	pu	uv	shug	su	ur	lists	exp'ments
Dictaphone .	—	8	10	25	22	13	19	19	116	11200
Telephone ..	23	30	32	20	20	15	14	14	168	15300
Air	9	25	24	20	20	14	14	14	140	12750
Wall	1	1	1	—	—	—	—	—	3	250
Table	1	6	6	—	—	—	—	—	13	1000
Lists	34	70	73	65	62	42	47	47	440	40500
Experiments	3400	3500	7300	6500	6200	4200	4700	4700	40500	

The Reagents were students, ranging from sophomores to post-graduates, drawn from Dr. Angell's large class in General Psychology.

In Section I, we shall first display some of the results obtained under the more varied conditions, and the results under the more constant conditions, when the syllable recorded contained an initial and a final consonantal sound; in Section II, the results obtained from recording syllables containing an initial or a final consonantal sound.

Results.

Section 1. Syllables with Initial and Final Consonantal Sounds.

"Chain"—Telephone. The "chain" list which contained 58 words and 42 nonsense-syllables was recorded from the telephone by the twelve reagents who had not heard or seen the list before. Out of the hundred monosyllables the lowest score in number of correct records was 34, the

[11] The "chain," "chack," and "pav" lists were given in 1913-14; the "pav" and "chack" lists in 1914-15; the "pu" and "uv" lists in 1915-16; and the "shug," "su," and "ur" lists in 1916-17.

highest 63, the average 51.3. It was likewise recorded by nine reagents who had dictated it through the telephone, the lowest score being 50, the highest 72, the average 66.7.

"Chain"—Air. It was recorded from dictation through the air (25 meters distant) by four students who had already recorded it once from the telephone and had dictated it once. The lowest score was 57, the highest 88, the average 68.5. It was also recorded from dictation through a closed door by one student who made a score of 21, and from dictation across a table by another who made a score of 64.

"Chack"—Telephone. In 1913-14, the "chack" list was recorded from the telephone by twelve students who had not seen or heard the list before, the lowest score (in per cent) being 18, the highest 64, and the average 33; also by six students who had recorded it once before, the lowest score being 6, the highest 60, and the average 38.3; and by six students who had dictated it before, the lowest score being 32, the highest 66, and the average 47.3.

"Chack"—Air. Six students who had not recorded the list before recorded from dictation through the air, the lowest score being 8, the highest 48, the average 30; five students who had recorded it once before made, lowest score 26, highest 48, average 34.4; six students after having recorded and dictated it before, made, the lowest score 44, the highest 70, average 57; and one student who had dictated it twice made 60.

In 1914-15 no student who recorded the list was permitted to see it; but each recorded it three or four times in the following order: Dictaphone, Telephone, Air, Table. In comparison of the results some allowance must be made for practice, since the reagents in the preceding year who had recorded the list before made a higher average score than those who recorded it for the first time. The advantage was about 5%, or about 17% of initial capacity. The results are shown in comparison with those of the preceding year in the following table:

TABLE XCIV.
The "chack" list.

	Dictaphone	Telephone	Air	Table
1913-14. Number of Lists.....	—	(12)	(6)	—
Lowest Score	—	18	8	—
Highest "	—	64	48	—
Average (%)	—	33	30	—
1914-15. Number of Lists.....	(8)	(5)	(6)	(6)
Lowest Score	0	10	8	52
Highest "	28	40	58	78
Average (%)	9.8	26.	30.7	64.7

"Pav"—Telephone. In 1913-14, the "pav" list was recorded from the telephone by twelve students who had not previously heard or seen it, making the lowest score of 15, the highest of 55, an average of 38.4; also by six students who had already recorded it once, making the lowest score of 16, the highest of 47, and an average of 34.2; and by nine students who had previously recorded and dictated it, making the lowest score of 26, the highest of 61, and an average of 41.3.

"Pav"—Air. It was recorded from dictation through the air by six students who had neither heard nor seen it previously, making the lowest score of 13, the highest of 59, an average of 37; also by three students who had recorded it once before, making the lowest score of 16, the highest of 45, an average of 30; and by eight students who had recorded and dictated it previously, making the lowest score of 45, the highest of 72, an average of 57.5.

In 1914-15 the "pav" list was recorded only by students who had not seen it, but most of them recorded it more than once, beginning with the dictaphone and following with the telephone, through the air, and across a table. The averages are shown in comparison with the foregoing in the following table:

TABLE XCV.
The "pav" list.

	Dictaphone	Telephone	Air	Table
1913-14. Number of Lists.....	( )	(12)	(6)	( )
Lowest Score	—	15	13	—
Highest "	—	55	59	—
Average "	—	38.4	37	—
1914-15. Number of Lists.....	(10)	(5)	(6)	(6)
Lowest Score	5	17	21	63
Highest "	15	45	59	79
Average "	11.1	31.6	37.8	70.8

In 1916-17 the "shug" list (which contained only the twenty simple consonant sounds) was recorded by students who had not seen it, taking their records in order from dictaphone, telephone, and air, with the following results:

TABLE XCVI.
The "shug" list.

	Dictaphone	Telephone	Air
Number of Lists.....	(13)	(15)	(14)
Lowest Score	3	2	7
Highest "	27	38	53
Average "	13.5	20.3	35.4

When we disregard the practice-effect of recording the lists, which would tend to raise the average, and increase the mean variation (MV) of the records from the telephone, air, and table, progressively, and collect the results just reviewed into aggregates under the respective heads, we get the following table which shows the number of lists, the average per cent of correct records in a list, and the probable error (PE)[12] of the average:

TABLE XCVII.
Per cent of Syllables Correct.

	Dictaphone	Telephone	Air	Table
Chack	(8) 9.8±2.16	(18) 36.8±2.78	(16) 30.2±1.98	(6) 64.6±2.56
Pav	(10) 11.1±0.51	(23) 35.2±1.70	(15) 35.9±2.47	(6) 70.8±1.21
Shug	(13) 13.5±2.16	(15) 20.3±1.68	(14) 35.4±2.51	

There were 400 records (50 in a list) from the "chack" list recorded by 8 reagents from the dictaphone, of which 9.8% were correct. The probable error of this measure is 2.16; which indicates that if experiments continue indefinitely under these conditions we may expect half of the averages of an equal number of lists to fall between 6.64% and 11.96% correct records. There were 23 "pav" lists (2300 syllables) recorded by 23 reagents from the telephone, in which 35.2% of the records were correct. The probable error of the average is 1.70; consequently should an indefinite number of returns from aggregates of 23 lists recorded from the telephone be obtained, we should expect half of their respective averages to fall between 33.5% and 36.9% correct records.

From this table we learn that when the reagent records syllables which begin and end with a consonant, under these varying conditions all of which were found by each reagent to be adequate for communication when the sounds came in an order to make sense, his sense of hearing enables him to get only a part of the syllables right; from the dictaphone less than 15%, from the telephone less than 40%, from the air less than 40%, and across a table less than 75%. Since this is all his ear can contribute, and since when the same sounds come in their accustomed order he can record all of them, his record of a communication must de-

[12] This probable error is reckoned on the mean variation. $PE_M = .845MV/\sqrt{n}$ ($MV = 0.7979\sigma$; *vide*, Davenport: Statistical Methods, 3d ed., p. 116). This procedure increases slightly the effect of the smaller deviations, but reduces considerably the effect of extreme deviations, and, for the purpose of the latter effect, is consistent with Yule (Theory of Statistics, 3d ed., p. 146).

pend upon what the mind contributes to the perception of the words for the difference; *i. e.*, from the dictaphone his mind contributes over 85%; from the telephone, over 60%; from the air, over 60%; across the table, over 25%.

If, then, we consider the amount of error in the recording of monosyllables [18] under these conditions adequate for communication, as a rough measure of the contribution which the mind makes to the perception of the words in the communication, we find the amounts in per cent according to the respective lists, as follows:

TABLE XCVIII.
Per cent of Mental Contribution.

	Dictaphone	Telephone	Air	Table
Chack	90.2	63.2	69.8	35.4
Pav	88.9	68.4	64.1	29.2
Shug	86.5	79.7	64.6	

These per cents, of course, refer to syllables. And since these syllables contain an initial and a final consonantal sound (excepting in the irregular "chack" list) there are two chances to miss a correct record by failing to perceive a consonantal sound. Let us see to what extent the ear can be relied upon when we consider the consonantal sounds.

Section II. Syllables with an Initial or a Final Consonantal Sound.

There were four lists of syllables which contained either initial or final consonantal sounds only: "pu," "uv," "su," and "ur." And if we take also the initial and final consonantal sounds in the "pav" and "shug" lists, we have altogether a large number of lists which may be considered for the purpose of determining what per cent of consonantal sounds can be correctly recorded under the three general conditions of communication.

The following table shows the list, the number of sets (of 100 syllables), the average per cent of correct records in a list, and the probable error of the latter value. Line 5 gives the aggregate number of syllables and the per cent correctly recorded; and line 6 gives the like values for a selected aggregate of syllables (those containing the simple consonantal sounds only) which was made for use in a following Section.

[18] The syllables in these lists begin and end with a consonantal sound, excepting the "chack" list, which is varied and contains some intermediate consonants.

TABLE XCIX.

Per cent of correct records of Consonantal Sounds.

	Dictaphone		Telephone	
	Intial	Final	Initial	Final
1. pav	(10) 43.1±1.2	34.4±1.6	(23) 62.0±1.1	58.0±1.7
2. pu, uv	(20) 39.0±1.3	18.7±0.7	(20) 46.9±1.6	38.0±2.0
3. shug	(13) 36.7±2.9	30.4±3.4	(15) 47.9±2.6	41.1±2.5
4. su, ur	(19) 38.2±1.6	37.1±1.4	(14) 60.4±2.7	57.3±2.7
5. Aggregate ..	(6200) 38.9	29.3	(7200) 54.5	49.0
6. Selected	(4490) 39.1	(4424) 34.3	(5656) 58.4	(5604) 55.6

	Air	
	Initial	Final
1. pav	(15) 71.0±1.7	51.8±2.2
2. pu, uv	(20) 68.7±2.1	55.0±2.7
3. shug	(14) 67.0±2.9	49.8±2.7
4. su, ur	(14) 70.0±2.6	62.2±2.4
5. Aggregate ..	(6300) 69.1	54.6
6. Selected ...	(5132) 72.8	(5088) 59.6

With the exception of such variation as may be expected under the variable conditions of our experiments,[14] a fair degree of consistency is to be seen in the values in the table. From the dictaphone records about 40% of the initial consonantal sounds were correctly recorded, and about 35% of the final; from the telephone, about 58% of the initial, and about 55% of the final; and from the air, about 70% of the initial, and about 60% of the final consonantal sounds.

Taking the complement of the per cent of correct records in the aggregated results as a rough measure of the mental contribution to perception of consonantal sounds we may estimate from the preceding table the values (in per cent) given below:

	Dictaphone	Telephone	Air
Initial Consonant	61	42	27
Final "	66	44	40

These per cents, however, are based upon a fairly even distribution of the various consonantal sounds. In the "pav," "pu," and "uv" lists, all of the more usual consonantal sounds, both simple (as, *t, sh, m*) and coalescent (as, *tr, spl, rd, lf, nch*) were included, and were presented with about equal frequency; in the remaining lists only the twenty simple sounds were included, and their frequency was uniform. The various consonantal sounds do not occur in English, of course, in this regular

[14] The "uv" list (line *2*) from the dictaphone, may have suffered from a disproportionate wearing down of the cylinder; and the "ur" list (line *4*) may have been pronounced in the air with emphasized distinctness.

way; some are much more frequent than others, and if we wish a measure of the mental contribution to the perception of English words we must take account of this fact. But in order to do so we must ascertain: (1) the per cent of error on each of the consonantal sounds, and (2) the relative frequency of these sounds in English.

Section III. The Respective Consonantal Sounds.

In order to simplify our inquiry we aggregated all of the records made from the twenty simple consonantal sounds shown in the table below. As shown in line *6* of Table XCIX above, we had from the Dictaphone 4490 such records of initial sounds, and 4424 of final sounds; from the telephone 5656 of initial, and 5604 of final sounds; and from the air 5132 of initial and 5088 of final simple consonantal sounds. All of the errors in these records were tabulated according to the simple consonantal sound which had been dictated and also according to the consonantal sound which was substituted for the dictated sound.

Table C shows (in per cent) the frequency of substitution for the respective sounds.

TABLE C.
Per cent of Substitutions.

Initial:	*p*	*b*	*t*	*d*	*ch*	*j*	*k*	*g*	*f*	*v*	*th*	*dh*	*s*	*z*	*sh*	*zh*	*m*	*n*	*l*	*r*
1 Dictaphone	63	40	66	53	61	45	57	53	86	89	90	80	72	76	59	88	51	50	33	14
2 Telephone	55	30	31	21	31	27	55	55	55	57	76	68	39	56	27	80	24	19	28	29
3 Air	33	25	14	8	21	24	17	21	38	58	50	51	14	34	32	73	16	13	17	16
Final:																				
4 Dictaphone	60	68	67	72	53	60	75	68	83	68	98	96	80	49	35	80	54	48	68	40
5 Telephone	59	41	37	35	31	47	56	42	65	59	92	91	47	45	31	70	29	26	37	22
6 Air	53	51	38	35	30	40	39	32	63	55	82	82	25	28	31	66	29	29	31	8
7 Frequency	2.0	1.3	10.	4.	0.6	0.1	2.0	1.7	2.6	1.0	1.8	1.8	5.7	2.2	0.3	—	2.3	6.9	3.6	6.1

These values are given to the nearest integral per cent. In line *1*, the sound of *p* was dictated from the dictaphone as an initial sound 235 times, and of the corresponding records 149 were wrong, making 63.1% of substitutions for that one sound; in line *4* the same sound was dictated from the dictaphone an equal number of times as a final sound, 59.6% of the records being wrong. The individual consonantal sounds were dictated through the telephone 301 times, and through the air 272 times; consequently the percentages in the table are reckoned upon these numbers.[15] The limit of chance deviation from these values is 6%, the

[15] A few exceptions may be noted; in the aggregates of initial sounds, *g*, *th*, *dh*, *sh*, and *zh*, were not presented quite as many times; and in the aggregates of

standard variation is 1.4%, and the probable error is 1%. Line 7 shows the relative frequency, estimated to the nearest tenth per cent from a tabulation of the letters of the alphabet in 5247 words (22,881 letters) in English text, which was made in another research.[16]

The amount of substitution for the various sounds is graphically shown in Plate XLIX. The heavy (upper) line represents the records

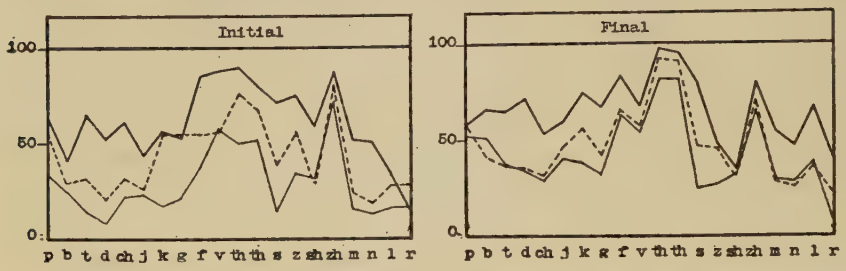


Plate XLIX.— Per cent of Substitution for the respective Simple Consonantal Sounds heard through the dictaphone (upper solid line), telephone (dotted line), and the air (lower solid line). (The second *th* is vocalized; elsewhere represented by *dh*).

from the dictaphone; the dotted line, from the telephone; the light (lower) line, from the air.

The order of the letters in both table and plate is that of Isaac Pitman, the celebrated author of the system of phonography which bears his name. It is based upon an analysis of "the English spoken language," and follows the physiological order of articulation: (1) labials, (2) lingo-dentals, (3) lingo-palatals, (4) gutterals; first come the surd and sonant mutes (whispered and voiced explosives), then the surd and sonant fricatives (whispered and voiced continuous sounds), and finally the nasals (voice emitted through the nose) and the liquids (so-called because of their facility in flowing into other sounds).

---

final sounds the last four sounds just enumerated were not presented quite as frequently. In all cases the per cent is reckoned upon the number of times the individual sound was dictated, however.

[16] The frequency of *p* was 1.956% of all the letters (vowels and consonants) tabulated; of *b*, 1.332%, etc. The frequency of *c* (2.88%) was divided equally between *k* and *s*. A third of the frequency of *s* was transferred to *z*; and the frequency of *th* (3.573%) was divided equally between the two sounds of *th* (*th* and *dh*, which is, of course, quite arbitrary, since perhaps 97% of all occurrences of *th* in English text has the vocalized sound, owing to the frequency of the words *the, this, that,* and perhaps 85% of its occurrences in names has the other sound, as in *Ruth, Dorothy, Katherine, Edith, Arthur, Martha, Elizabeth, Timothy, Bertha,* etc. The *zh* is so rare that we may disregard it.

The values in Table XCIX indicate that the final consonantal sounds are as a whole more difficult to record correctly than the initial, and an

examination of the curves in the two parts of Plate XLIX will suggest that the final sounds as a whole differ from the initial. This is true both physiologically and phonetically. A surd mute as initial (as in *tea*) for example, is produced by an explosion of the breath from the barrier formed by the tongue and teeth which immediately issues in vocalized sound, while as final (as in *at*), it is produced by an occlusion of the vocalized breath followed by an explosion of breath which is not vocalized. And it is quite certain that the three general conditions varied not only with respect to the relative indefiniteness of the respective sounds in each respective class (initial or final) but also with respect to the relative indefiniteness of the initial and final sounds of the respective letters. For example, the final mutes which were only moderately more difficult than the initial mutes to record from the dictaphone and the telephone were much harder from the air; the two initial sounds of *th*, which were moderately more difficult than the corresponding final sounds to record from the dictaphone, increased in relative difficulty from the telephone, and still more from the air. Final *r* was much more obscure than initial *r* from the dictaphone, but was less obscure from the telephone and the air. Final *v*, which from the telephone and the air was about equal in indefiniteness to initial *v*, from the dictaphone is relatively much less indefinite.

Line 7 in Table C gives the estimated frequency of the sounds in English text without respect to whether they are initial or final in the syllable. It will therefore be necessary to assume a ratio for the whole of the sounds. Suppose all of the sounds occur as often in the initial position as in the final; then, by multiplying each value (per cent) under the respective letters by the corresponding frequency (also per cent), and aggregating the products for the six respective lines, we get

TABLE CI.

Per cent of error in the perception of English Speech.

	Dictaphone	Telephone	Air
Initial	31.75	20.45	11.45
Final	36.20	23.46	20.02
Average	33.98	21.96	15.74
Consonants	51.50	33.25	23.80

If all consonantal sounds recorded from the dictaphone were initial sounds in English speech, the ear, under the condition of our experiments, could not be expected to identify 31.75% of all the sounds (consonantal and vowel) in the dictation; if they were final sounds the amount would

be 36.20%; or, on the assumption that they were equally divided between initial and final sounds, it would be 33.98%. The corresponding values for communication through the telephone and across 25 meters of airspace are 21.96% and 15.74%. And since it is only the consonantal sounds in English speech (about 56% of all sounds) which fail the ear, the per cent may be expressed in terms of the consonantal sounds as distributed in English speech (51.50%, 33.25%, and 23.80%, respectively).

Our conclusion from these results, then, is that although our students could satisfactorily understand the communications through the dictaphone, the telephone and the air, when the sounds came in their accustomed order, and could record all of the sounds without error, *they could not hear definitely enough to identify a half of the consonantal sounds through the dictaphone, a third of them through the telephone and a quarter of them through the air.* If they *thought* they heard all of the consonantal as well as the vowel sounds, or, if they thought their perception of the words depended upon their hearing them, they overlooked entirely the substantial contribution which the mind was making to their perception.[17]

[17] On these more subtle psychological matters the layman is always skeptical. He, however, can provide himself with a demonstration by having his neighbor dictate to him in his customary telephonic manner of speech one of the lists of nonsense-syllables (in Appendix A) over the telephone. Or, if he has access to a dictaphone he can dictate a list to the machine himself and immediately afterward transcribe it.

A mature student (Mr. B.) who was skeptical dictated (on December 6, 1915) the "pu" list to the dictaphone, and immediately afterward transcribed it. He got 37% of the syllables correct; other students who later transcribed from the same record got 32%, 34%, and 41% correct, respectively. An experienced teacher of stenography and typing (Mr. H. H. S.), who also does court-reporting and is accustomed to transcribing from the dictaphone, transcribed (on August 3, 1917) the "su" list which the writer had just dictated. He got 42% correct.

In Dictations I and II on the dictaphone, in the following Division, was the name, P. F. Venn; the closest records were: P. F. Bent, P. F. Venn, P. F. Bent, C. F. Vent, C. Denning, and I. Grant. And in another, were ten names, the following four of which being the more nearly correctly transcribed (the first line gives the dictation, the others the transcriptions):

P. M. Gray	D. V. Skake	S. P. Gates	V. N. Jack
T. M. ——	T. C. Skate	F. C. Gates	G. M. Jacks
P. M. Gray	D. P. Skate	M. G. Gates	G. M. Jack
		S. C. Gates	P. M. Jack
		F. P. Gates	

Whether the reagent is skilled or unskilled with the instrument (telephone or dictaphone) he cannot hear many of the consonantal sounds, and must therefore depend on the context for them.

If the student is asked whether he heard all the sounds of the communication in English speech which he has satisfactorily received, he is apt to reply confidently that he did so; that he heard what was said,—of course he heard the sounds. But, if he is pressed to reply with respect to all of the sounds, and if he is cautious, his confidence is likely to weaken and he will reply that to the best of his belief he heard them all; he is not prepared to make a definite scientific observation on the matter.

The question then arises as to whether these sounds are really not essential to communication and are elided in speaking or are neglected by the ear, *or are really heard as other sounds.*

To throw light upon this question further experiments were performed. Their results are detailed in the following Division.

DIVISION II.—SIMULATED ENGLISH TEXT.

It has been noted that the various sounds which were substituted in the recording for the dictated sounds have been tabulated so as to show not only the frequency with which the respective dictated sounds have been missed, and the frequency with which the respective sounds have been substituted for others, but also the frequency with which each particular sound has been substituted for each other. For example, *p* was dictated by the dictaphone as an initial sound 235 times; it was missed in the recording 149 times; substituted for it were *b* 5 times, *t* 40, *d* 0, *ch* 2, *j* 3, *k* 34, *g* 2, *f* 5, *v* 3, *s* 1, etc. And *p* was substituted for other simple sounds 162 times: for *b* 11 times, *t* 7, *d* 3, *ch* 4, *j* 0, *k* 11, *g* 4, *f* 38, *v* 18, *s* 15, etc. The tabulation makes a coördinate table from which one can readily learn what sounds were most frequently substituted for what others, and thus be able to arrange some English text by supplanting the consonantal sounds with the most frequent substitutions for them which, when dictated under the same conditions under which the substitutions had been made, should so strongly suggest the English text to the reagent that some of it at least might get into the record not as "garbled" English but as correct English,—even though the reagent is instructed to faithfully record exactly what he hears.

For the preparation of a dictaphonic record the tables of substitutions of initial and final simple consonantal sounds show that the sounds in line *1* of the following table should be replaced by those in line *2* if initial, or by those of line *3* if final:[18]

(1)----	p	b	t	d	ch	j	k	g	f	v	th	dh	s	z	sh	m	n	l	r
(2)----	f	m	p	z	sh	zh	t	d	th	dh	v	v	th	dh	ch	n	m	r	l
(3)----	b	g	k	t	j	zh	p	d	v	l	—	—	z	dh	zh	n	m	v	v

Suppose the text to be forced by suggestion into the record is "The aim of this exercise is . . . ," it should be dictated to the dictaphone as "Ve ain ol viz epsevthidhe idh . . . ," observing the vowel sounds, the accent, the inflection, and the time-elements of the syllables of the original as faithfully as possible.

Such a dictation was made to the dictaphone (November 29, 1916)

[18] Provided we limit the selection to substitutions with the highest frequency, and disregard some close seconds. The vocalized *th*, as in *the*, is represented here also by *dh*.

while the prospective recorder was sitting just beyond the closed door taking the dictation through the door; he did not know the dictation was being put on the dictaphone, and had been instructed to record faithfully what he heard. Line *1* of the exercise printed below is the dictation;[19] line *2* is the student's record of the dictation through the door; and line *3* is his record from the dictaphone immediately afterward:

(1) Vu ane oth vith etserthith ith pu cho kow tersept ith
(2) The aim of viv etxp iv a cho cow persa iv
(3) The aim of this exercise is to show how perfect is

vu zitpasome ath am imspranemp oth tonnuniprashum. Vu thoith
va sitvasone av am imspament of The thoi
the dictaphone as an instrument of The voice

theps vu ziaspran oth vu kramthnipper imko thivrashum; vu
fef va viasfram of va krapniber imper vibration; the
the diaphragm of the transmitter into vibrations. The

retordeen bethith oth vu kranthnipper tomtherps vu thoumb oth vu
retortion devife of a transnipper converts sounds of
recording device of the transmitter converts the sound of the

thoith impu imbemkachums om vu wax thilimber; vu meedle oth
voice in vu of wax filament; va meedle of
voice into indentations on the wax filament. The needle of

vu refrogoother thollows vu drooth oth imbempations, amb vuth
reprothuser tholoes thu of impentation, am vu
the reproducer follows the grooves of indentations, and thus

lekrogootheth vu lambwaj vap yuth sporeb im vu wax; amb vu
the proothes thu language that is thored in thu wax; an thu
reproduces the language that is stored in the wax; and the

spubemp leporbs vith rambwaje im ripim. Vu atpurafy oth vu
records vif rampage im writing. Vu atrophy of vu
records this language in writing. The accuracy of the

spubemp's reporg illusprapf vu leliamiliky oth vu zitpathome
s bepor illustrats va liability of thu zitrazone
(hearer) illustrates the reliability of the dictaphone

ath am imsprunemp for sporim amb leprobuthim lambwaje.
av am instrument for forming and distributing language.
as an instrument for forming and introducing language.

Now, the record in line *2* shows that the reagent understood the instructions and that he exercised a fair degree of skill in following them.

[19] The substitutions were determined from only a partial tabulation of the data which have since been completed, and differ from those suggested above.

So clearly was the dictation "garbled English" that after the third word and until the last phrase it did not even seem to simulate English text having sense. The dictation from the dictaphone, on the other hand, seemed to be English text, expressing meaning, but at times inarticulately. The recorder was entirely incredulous when told that the two records had been made from the same dictation, and was finally convinced only by holding a typed copy of the "garbled" edition while listening again to the dictaphone; even then, to his bewilderment, the dictaphone occasionally forced the English upon him.

In experiments of this nature conducted with the dictaphone during the last two years we have obtained data which are pertinent to the question raised above concerning the reality of the auditory impressions of consonantal sounds that are not there. These data may be more conveniently presented in five parts, since the experiments were conducted with five distinct texts.

Dictation I.

The first dictation to the dictaphone was transcribed by nine students and critically examined and challenged by ten students. The procedure was as follows:

(1) The student was told to record from the dictaphone exactly what he hears. He is to have two tries at it, and in the first trial space should be left for what is missed in order that it may be filled in in the second trial without needless copying of text already recorded. He was given a pad of composition paper and a pencil.

(2) The dictaphone was started. With the receiver at his ear, he heard from the dictaphone the instructions repeated, and a signal, "Ready." He then began transcribing. After the dictation was finished, the carriage was returned to the beginning and the process was repeated.

(3) A typed copy of the English text, which the dictation was intended to simulate, was supplied the reagent, with instructions to follow it with his pencil-point, letter by letter, while listening again to the dictaphone, and to make a short vertical stroke under any letter the sound of which did not come true. In a second trial, he was to record, if possible, under each challenged letter the sound he heard instead; finally, to estimate the per cent of consonantal sounds that did not come true.

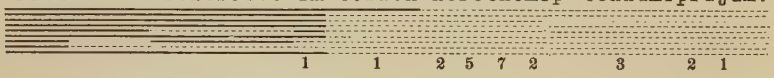
(4) He was then given a typed copy of both English and "garbled" texts, in interlinear arrangement as shown below, that he might examine each alternately while listening again to the dictaphonic record.

Below are given (1) in line *1*, the English text; (2) in line *2*, the

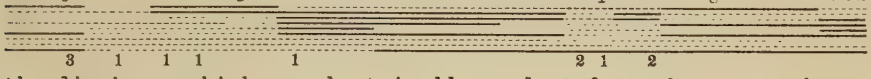
"garbled" text which was dictated to the dictaphone; (3) in the underscoring, the parts of the English text which appeared in the records of the nine reagents who transcribed; and (4) in the digits, the number of challenges made by ten reagents upon the English-text sounds immediately above the digits.

Dictation I.

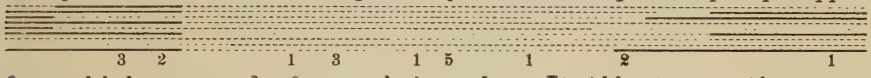
(1) This is an exercise in correct telephonic communication.
(2) Zith iv am echerfive im tozzek kerethomip tonnumiprajum.



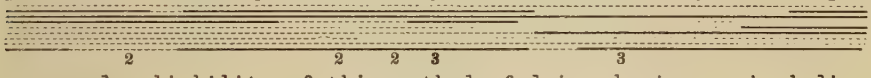
Sounds are presented to the receiver and set up vibrations of
Founj az hefempeb ku zu wefether am fep ut thigrachumv oth



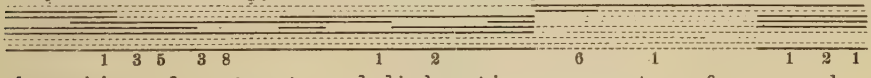
the diaphragm which are electrically conducted to the transmitter
vu giathlan wix al eregklitary tombumteb pu zu prampsnipper



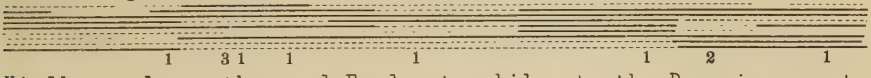
from which a record of speech is made. It illustrates the accu-
prun wits a breporg ob steex ij nabe. Ik ilduspraph vu atpu-



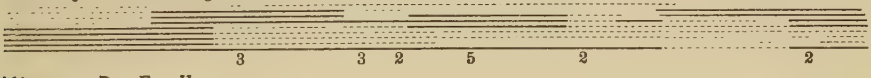
racy and reliability of this method of doing business, including
rafy am weriaviripy oth zif nesob oz goim vithmef, implubim



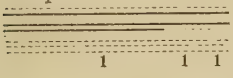
the making of contracts and diplomatic agreements; for example,
zu nating oz tomprax am giflonapip adreenemps; thor exantle,



Kindly send one thousand Ford automobiles to the Prussian agent,
Timbly femb wung southumb Thorb okonovilv pru vu Prussian azemp,



Mister P. F. Venn.
Nisper P. F. Venn.



It is true that the English in the instructions from the dictaphone was generally understood upon the first hearing, and that the "garbled"

text presented difficulties in many instances insuperable,[20] but the "garbled" version did in some measure force the English text into the transcription, and in a much less measure suggested other content which was uniformly English and not nonsense-syllables. As the underscoring shows, all nine reagents transcribed the first word, seven the second, third, fourth, and fifth words, one the sixth word, and none the following two words. The relative position of the ruling identifies the reagent, and shows great variability among the reagents.

The digits show that, when the English text was held and critically examined while the reagent was listening the third and fourth times to the dictaphonic record, the sounds challenged were mostly those that failed in the transcription. These challenges often resulted from variance between the English recorded in the transcription and the typed copy. Some of these variant editions follow: Instead of receiver, Cæsar; telephonic, every comic; contracts, thumb-tacks; including the making, improving the mating; set up vibrations of the diaphragm, head up by greatness of the diaplan; vibrations of the diaphragm, migration of the giant-land; contracts and diplomatic agreements, subtract and it will happen at Reno; kindly send one thousand Ford automobiles to the Prussian Agent, Mr. P. F. Venn, kindly send one dozen of the nodules to the president, Mr. I. Grant.

The Introspections that were written by some of the reagents, and the numerical calculations based on these performances will be given attention after the exhibition of the other dictations.

Dictation II.

In Dictation II the simulated English text differs slightly from that of Dictation I, and the substituted sounds are almost completely changed. Only three students transcribed from the record, and five critically examined the typed English text while listening the third and fourth times to the dictaphonic record and challenged such sounds as did not come true. The general procedure was the same as that described above.

The following shows in line *1* the simulated English text, in line *2* the dictation, in the underscoring the English text which appeared in the transcriptions of three reagents, in the digits the number of challenges made by five reagents on the sounds of the English text immediately above.

[20] The major part of the difficulties presented were owing to failure in the dictating to preserve the accent, inflection, vowel sounds, and temporal elements, of the English text, rather than in the substituted consonantal sounds, and to the wearing down of the wax record through ten or a dozen repetitions.

Dictation II.

(1) This is an exercise in correct dictaphonic communica-
(2) Vith ith am etsevfith iv tovekt stipkasozit tonnunita-

1 2 1 1 1 1 4 2

tion. Sounds are presented to the receiver and set up vibrations
shung. Foungth av frethemted ku vu vefether amd fep ut thifrations

1 1 1 1 2 1

of the diaphragm, which make indentations on the wax cylinder.
oz vu stiaspram, wits nate izstenkationth oz vu whaps firinger.

1 1

The transmitter reproduces these vibrations in the speech of
Vu kranthnipper vrefrothutheth veth thiflashumth im va steets oz

1 1 1

which the written record is made. It illustrates the accuracy
wix vu vikkem vreporg ith nage. Ik imusprapf vu atpurafy

1 1 1

and reliability of this method of doing business, including the
amd veliamiriky oz vith nefob oth stewing mithmeth, implubing vu

1 1 2 1 1 1

making of contracts and diplomatic agreements; for example, Kindly
zaping oz tomprax amd stipronakip abreenemps; sor etzantle, Timbly

1 2 2

send one thousand Ford automobiles to the Prussian agent.
femj wung fouthumb Sorp akonovilv ku vu Prussian azempt.

1 1 1 1 1

The underscoring indicates that the "garbled" text here forced into the transcriptions a relatively greater amount of the English text.

Dictation III.

In Dictation III the English text varies a little from the preceding, and the substitutions are entirely changed. Two purposes governed the dictation: (1) To express all the consonantal sounds by the use of but six simple sounds (*t* for t, p, k; *d* for d, b, g, dh, v; *s* for s, f, th; *z* for z, dh, j, v; *n* for n, m, ng; *r* for r, l, v); and (2) to reduce the stimuli to nearer the threshold of perception by holding the mouth-piece a little farther from the lips while dictating. Unfortunately the record was too dim for transcribing, but, owing to the momentary scarcity of blank records, it was nevertheless used. Two reagents attempted to transcribe it; seven critically examined the sounds while holding a typed copy of the English text and challenged those that did not come true.

Dictation III.

(1) This is an exercise in correct telephonic communication.
(2) Dis is an etsersize in torrett teresonit tonnunitatrun.

Sounds are presented to the receiver and set up vibrations of the
Sounds are tresented to da reseider and set ut didratruns oz za

diaphragm, which make indentations on the wax cylinder and thus
diasran, rits nate indentatruns on za rats syrinder and dus

actuate the transmitter to produce speech. This is then recorded.
attuate du transnitter to troduse steets. Zis is zen retorded.

It illustrates the accuracy and reliability of this method of do-
It irrustrates zu atturacy and reriadirity oz zis nesod oz do-

ing business, including the making of contracts and diplomatic
in dizness, intrudin zu natin oz tontratts and ditronatit

agreements; for example; Kindly send one thousand Ford automo-
adreenents; sor etzanta; Tindy send one sousand Sord autono-

biles to the Prussian agent.
dires to zu Trussian azent.

Dictation IV.

(1) To be or not to be, that is the question: Whether 'tis
(2) Ku be or mop ku be, vat iv vu twestion: Wever 'kiz

nobler in the mind to suffer the slings and arrows of outrageous
modler im vu nind ku fuffer va slings amd arrows of ouprageous

fortune, or to take up arms against a sea of troubles, and, by
sortune, or ku kape up arns abenst a fee oth krubbles, amd, by

opposing, end them.
oppoving, emd vem.

Dictation IV.

In Dictation IV the general procedure, with an exception which applies to only three of the reagents, was the same as described above; the chief difference in the dictation is that what was expected to be familiar English was simulated in the "garbled" text, and only half of the consonantal sounds were changed. Eight reagents attempted transcribing; and eight critically examined the typed English text while listening to the dictaphonic record and challenged the sounds that did not come true.

As was to be expected much more of the text was transcribed, and, as will be shown later, a relatively larger ratio of the substitutions were found in the transcriptions. But for some of the reagents the decrease in substitutions did not result in more complete transcriptions, since they were unacquainted with Hamlet. One such transcription, because it illustrates so well the force of the normal tendency to convert likely stimuli into language, even in the face of the adequacy of the stimuli (many of them not substituted) for the perception of other persons, is given in full (line *3*):

(1) To be or not to be,that is the question: Whether 'tis
(2) Ku be or mot ku be, vat iv vu tweetion: Wever kiz
(3) To be or not to be,that is the question: Whether in tears

nobler in the mind to suffer the slings and arrows of outrageous
modler im vu nind ku fuffer va slings amd arrows of ouprageous
or in sunshine that suffer such things and arrows of outrageous

fortune,or to take up arms against a sea of troubles, and,
fortune,or ku kape up arns abenst a fee oth krubbles amd
fortune,or the fate of harm, repent the sea of troubles, and

by opposing, end them.
by oppoving, emd vem.
while upholding repent them.

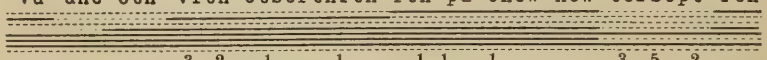
This reagent in his critical inspection of the typed English text while listening to the record for the third and fourth times, could only hear "sunshine" for "the mind," "these things" for "the slings," and "upholding" for "opposing," in conformance with his transcription, although the few substitutions would not, as the above text shows, support his auditory impression. He heard consonantal sounds that were not there.

The respect in which the procedure as observed by three reagents differed from the foregoing was in interpolating a step between the critical inspection of the typed English text for the purpose of challenging each sound that did not come true, and the critical inspection of the inter-

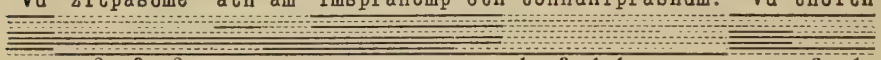
linear English and "garbled" texts: the reagent critically inspected a typed copy of the "garbled" text, while listening to the same dictaphonic record, and challenged each sound that did not come true. The wax cylinder had been ostentatiously removed, put in a box, and returned, and the student usually did not suspect that he was listening again to the same record. As before, he took two tries, first marking the unsatisfactory sounds, and then identifying "substitutions."

Dictation V.

(1) The aim of this exercise is to show how perfect is
(2) Vu ane oth vith etserthith ith pu chow kow tersept ith

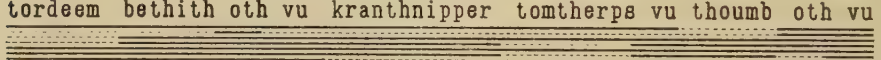


the dictaphone as an instrument of communication. The voice
vu zitpasome ath am imspranemp oth tonnuniprashum. Vu thoith



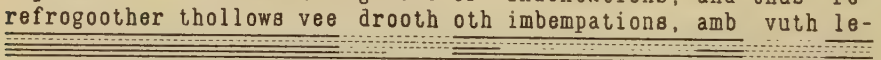
sets the diaphragm of the transmitter into vibration; the re-
theps vu ziaspran oth vu kramthnipper imko thivrashum; vu re-

cording device of the transmitter converts the sound of the
tordeem bethith oth vu kranthnipper tomtherps vu thoumb oth vu

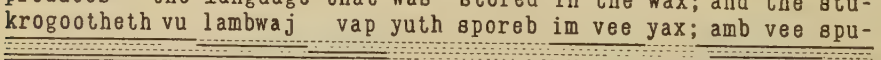


voice into indentations on the wax cylinder; the needle of the
thoith impu imbemkachums om vu yax thilimber; vu meedle oth vu

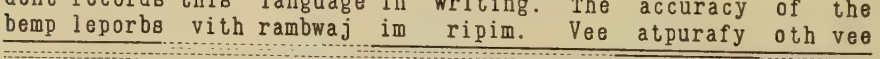
reproducer follows the groove of indentations, and thus re-
refrogoother thollows vee drooth oth imbempations, amb vuth le-



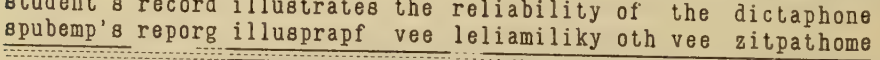
produces the language that was stored in the wax; and the stu-
krogootheth vu lambwaj vap yuth sporeb im vee yax; amb vee spu-



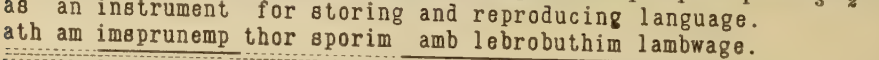
dent records this language in writing. The accuracy of the
bemp leporbs vith rambwaj im ripim. Vee atpurafy oth vee



student's record illustrates the reliability of the dictaphone
spubemp's reporg illusprapf vee leliamiliky oth vee zitpathome



as an instrument for storing and reproducing language.
ath am imsprunemp thor sporim amb lebrobuthim lambwage.



The reagent whose transcription is presented on page 393, made the challenges quoted there and three others, on the English text; but he made thirteen challenges on the "garbled" text, and the "substitutions" that he identified conform to the English text which he had just finished with. The performance of the two other reagents with the "garbled" text was precisely similar. The three reagents together made 33 challenges, specifying the "substitutions," and 31 of these "substitutions" conform to the English text. Those 33 consonantal sounds were heard, under conditions of critical examination, yet they were not there!

Dictation V.

For the purpose of trying out this last feature of the experiment under conditions as to text and substitutions uniform with Dictations I and II, Dictation V, which contains only a few improvements in substitutions, was made. Eight reagents attempted transcribing and eight reagents challenged sounds in both the English and the "garbled" texts. The exhibit, on page 394, shows in the same way as before the English text which the "garbled" text forced into the transcriptions, and the challenges on the English text.

The underscoring indicates that considerable of the English text was forced into the transcription by the "garbled" text, and the digits indicate that relatively few of the sounds in the English text were challenged when the dictaphonic record was inspected for this purpose.

Numerical Results.

We may now put some of the numerical calculations together. The extent to which the "garbled" text forced the corresponding sounds of the English text into the transcriptions, may be determined numerically if we consider the number of such cases of substitution in relation to the possible number. For example, nine reagents endeavored to transcribe Dictation I, in which there were 152 substituted sounds, making it possible to transcribe 1368 substituted sounds; 502 of these sounds being actually transcribed, the per cent of substitutions forced into the transcription is 37.

Table CII gives these values for each of the five dictations.

It is seen that in Dictations I, II, and V, in which the substituted sounds were close to 85% of all of the consonantal sounds, and the text was fairly difficult with respect to content, from a third to a half of the

TABLE CII.

English Sounds forced into the Transcriptions by the "garbled" text.

Dictation	I	II	III	IV	V	I, II, V
1. Substituted Sounds	1368	474	164	272	1480	3322
2. Transcribed	502	282	32	193	721	1505
3. Per cent	37	59	19	71	49	45

substitutions were replaced by their corresponding sounds of the English text in the transcription. In Dictation IV, in which only 51% of the consonantal sounds were changed, and the text was more familiar, 71% of the substitutions were transcribed as their corresponding sounds in the English text.

Of course, it was not the substituted sound that exerted the force toward its corresponding sound in the English text; the general semblance of the "garbled" text to the English text furnished the force. But the indefiniteness or the ambiguity of the auditory impression of the substituted sound permitted the transcription. The substitutions in the "garbled" text were not heard, were disregarded, or were heard as other sounds.

In a similar way we may determine what per cent the number of challenged sounds in the English text is of the number of substituted sounds.

Table CIII shows in line *1* the product of the number of consonantal sounds and the number of reagents who inspected the English text for the purpose of challenging the sounds that did not come true; in line *2*, the like aggregate number of English sounds which were changed in the "garbled" text; in line *3* the per cent of substituted sounds; in line *4*, the aggregate number of sounds in the English text that were challenged as not coming true; in line *5*, the aggregate number of challenged sounds in the English text that had been changed in the "garbled" text; and in line *6*, the per cent of substituted sounds that were challenged.

In the second line we have the number of critical judgments passed

TABLE CIII.

Challenged Sounds in English Text.

Dictation	I	II	III	IV	V	I, II, V
1. Consonant Sounds	1710	915	1197	536	1736	4361
2. Substituted Sounds	1520	790	574	272	1480	3790
3. Per cent	89	86	48	51	85	87
4. Challenges of English	132	45	39	56	160	337
5. Challenges (net)	129	44	29	44	143	316
6. Per cent	8.5	5.6	5.1	16.2	9.7	8.4

upon the substituted sounds, each judgment consisting of two observations while the reagent held before him the English text; and from the last line we learn what per cent of these substituted sounds were challenged,—usually less than 10%. What of the other 90%? There is no question of their being disregarded; there is no question of their being unheard,—*they came true to the sounds in the English text; they were heard as other sounds.*

Some of the students who critically examined the English text of Dictations I, II, III, and V, for sounds that did not come true expressed in per cent their estimate of that number (Line *3* in Table CIII gives the real per cent): Dictation I: 11, 20, 5, 10, 10, 15; Dictation II: 12, 7; Dictation III: 6, 8, 3; Dictation V: 35, 14, 25, 9. Their estimates were a little high considering the number of challenges, but they serve to support the statements italicised above.

The students who critically examined the sounds in the English text of Dictation V also critically examined the "garbled" text. In each case the wax cylinder had been removed from the dictaphone, put in its box, and returned in place, and the reagent did not know that he was hearing the same record while he was examining the sounds of the "garbled" text. Two of the reagents, however, declared that all of the consonantal sounds were wrong,—that this record also gives the English text. They were pressed to nevertheless specify the sounds that do not come true. Others were able to hear many of the sounds of the "garbled" text true. But challenges of all reagents were more frequent; altogether 562 as against 143 of the English text. In spite of the fact that the reagents looked at the typed characters in the "garbled" text while examining those identical sounds on the dictaphonic record, then, 562 or 38% were challenged as not coming true, and 95% of the dictaphonic sounds corresponding to the challenged text which were identified were identified as the sounds in the English text. *Half of the challenged sounds, which were true to the "garbled" text before the eyes of the reagent, were identified as other definite sounds.*

Five of the reagents gave in per cent their estimate of the dictaphonic sounds which were not true to the "garbled" text: 100, 100, 60, 33, 25. They were all true to the "garbled" text.

Introspections.

In order that some contribution may be made from the subjective side of these experiments with the dictaphone, we shall let some of the reagents speak for themselves:

(1) In the first passage [Dictation I, English tex.] I think there are about 10% of consonants which are not distinctly spoken.

In the second exercise [Dictation II, English text] I think there are about 12% of the consonants which are not clearly pronounced.

In the third exercise [Dictation III, English text] I think there are about 8% of the consonants which are not clearly pronounced.

After seeing the copy of what is really dictated to the machine [the "garbled" texts], and then hearing the machine, it is easy for me to realize that the consonants on the [dictaphonic] record are changed, but at first I imagined that the words were correct, that is, were as they are used every day.

November 22, 1915. D. G.

(2) When the dictaphone was first presented to me and I listened to the record, there was nothing but a jumble of sounds; but after a few minutes I could catch a few intelligible words. Then, upon the second trial, I was able to understand many more words, but still there were quite a few which I could not understand. It seemed as if the accent was being placed on some other part of the word than the right place. . . .

Then a paper [English text] was given me showing the real words and after that it [the dictaphonic record] was easily understood. Then a third paper ["garbled" text] was given me showing wherein our difficulty was. . . . It proves that the hearing is not as accurate as one believes, and speech has to do with many senses. Then again I discovered I was not depending on hearing, but the mind, and with each sound I tried to recall some word like it.

November 24, 1915. V. C.

(3) From this experiment I find that my hearing is not as acute as I believed it. On examination I found that *v* sounded like *th* and other equally absurd interchanging of consonants, but in many cases I found that I was not depending on my ear alone for the record of the dictaphone, but more on my mind—what the word sounded like, what it should be. I was surprised most to find that such a variety of consonants could sound like *one* on the dictaphone. And yet with two written examples [typed English and "garbled" texts] before me I could make it [the dictaphonic record] sound like the one or the other as I chose. This goes to prove, I believe, that speech has to do with other senses besides hearing,—habit, experience, and the like, come in to help in distinguishing a word.

November 24, 1915. D. H.

(4) On the paper [transcript from Dictation IV] "To be or not to be," it will be noticed at the close that the words I took down are entirely different from those which appear on the typewritten sheet [English text]. On going over the record, at the same time following the typed sheet, the sounds came to me exactly as they are on the sheet. It seems as though the typed sheet suggested and helped me to hear it as it is on this sheet.

As for the sheet marked (2) ["garbled" text], as I followed it with my eyes while listening to the record, I tended to fall into the way of hearing the words as they appear on this sheet; that is, with a few exceptions. (I have marked those.) Without any effort I could follow them in this manner. But in the case of "iv" for "is," I could not make myself imagine anything other than "is." [It was "iv."]

November 29, 1916. H. B.

(5) I listened to the record on the dictaphone machine [Dictation V] and wrote down the impressions I got from the voice. I had considerable difficulty in recognizing any words, and only recorded a few.

I was next given a sheet of paper [English text] containing several sentences, and I watched this wording carefully as I again listened to the voice of the record. This time I placed beneath the words any sounds I received from the record that didn't correspond to those typed on the paper. Very few of these were recorded, as the voice seemed to speak just what was on the paper.

I was next given a paper ["garbled" text] containing a lot of meaningless syllables so constructed that they nearly corresponded in sound to the impressions of the words I seemed to get from the machine. I watched these syllables as I listened to the machine, and recorded below them what I heard that didn't correspond to the syllables. I again seemed to hear the words that were on the first paper [English text].

I was then told that the voice pronounced, not the words [English text], but the meaningless syllables ["garbled" text] which were very much like the words.

I was then given a paper with both the words and syllables upon it, and after a little practice I could, by covering up one of the lines, hear the other pronounced by the voice.

In this latter procedure, I took a portion of both words and syllables which resembled each other in sound, which was as follows:

the	voice	into	indentations	on	the	wax	cylinder
vu	thoith	impu	imbemkachums	om	vu	yax	thilimber

By covering first one line and then the other I could hear each part as I followed it; that is, all except the last two words, "wax cylinder"; these I could not hear as the meaningless syllables below them, but the words I heard in each case, although it was the meaningless syllables below that were really recorded [on the dictaphonic record].

December 7, 8, 1916. R. R. S.

(6) In listening to spoken words, the effort of the mind is to extract the possible sense which the sounds uttered may be supposed to hold, in all cases where the pronunciation is indistinct, incomplete or uncertain. This was shown to me in the experiment of this afternoon, when I copied down what I thought I had heard from the dictaphone. The sounds of the words were not correct, but so far as possible, I seem to have striven to make them into words and intelligible combinations of words. This was true also when the garbled words [Dictation V] were dictated to me by Professor Coover, he being separated from me by a closed door. When I directed my attention upon the words as I thought they should be, I felt much more certain that they were being thus correctly pronounced; but when I concentrated on the garbled version, I began, with but few exceptions, to hear the mangled words much more clearly and unmistakably.

November 29, 1916. J. S.

(7) The first time I listened to the [dictaphonic] record [Dictation V], it was hard to associate the sounds to words which formed a complete sentence. But with the second trial I had more in mind what words were to be used, and I heard most of the sounds as definite words, although they were not clear.

When watching the first typed sheet with the words [English text], it [the

voice] seemed to me to conform more to the typed words, although there was much not clear.

When watching the second typed sheet with the sounds ["garbled" text], I felt that my previous judgment was very poor, for the sounds seemed to conform more to the second sheet.

I was very much surprised to know that the record [Dictation V] was dictated from the second sheet of sounds. I had believed that it was dictated from the first sheet of English words. The record seemed to sound more like the English words than the other. I imagine this is because I am more accustomed to the English words than the other rather odd combination of letters.

December 4, 1916. F. M. C

CONCLUSION.

When nonsense-syllables are recorded from the dictaphone, from the telephone, or at a distance of twenty-five meters, under conditions adequate for communication, the records are found to be only partially correct; if the syllables have both an initial and a final consonantal sound, the respective numbers of correct records are less than 15%, 40%, and 40%; if the syllables have only an initial consonantal sound, 39%, 58%, and 73%; if they have only a final consonantal sound, 34%, 56%, and 60%. And if the per cent of error on each of these simple consonantal sounds is considered in relation with the relative frequency of each in English speech, we find that of all the consonantal sounds in speech heard under the three general conditions which we have employed 51.50%, 33.25%, and 23.80%, respectively, may be expected to fail the ear. In other words, under these conditions of communication speech is readily understood, and all of the consonantal sounds in its composition could be correctly recorded; but if these sounds are pronounced in haphazard order, only half of them can be identified from the dictaphone, a third of them from the telephone, and a quarter of them from the air, by the ear alone. To this extent may the consonantal sounds be too indistinct for identification and yet contribute to satisfactory communication; to this extent, consequently, is the recipient of the communication exposed, under favorable conditions in context, to error. Reliance upon his ears would be precarious.

Although in the perception of English speech some sounds are commonly lacking by reason of elision in pronunciation,[21] and other relatively unessential sounds are disregarded, some consonantal sounds are also falsely heard, even when critically examined.

When 85% of the consonantal sounds in a more or less unfamiliar and technical sample of English text had been changed to the sounds most frequently substituted for them, and the "garbled" text thus composed had been put on a dictaphonic record, transcriptions from that record produced 50% of the simulated consonantal sounds of the English

[21] Especially obvious in the language of the street: Godda-macha-bowcha? (have you got a match about you?); c'meer (come here); hoo-zat? (who is that?); sry (that is right) or saw-ry (that is all right).

text. When the record was heard twice again while the reagent critically inspected the sounds in a typed copy of the English text for the purpose of challenging any sound that did not come true to the copy, only 10% of the changed consonantal sounds were challenged. When the sounds in a typed copy of the "garbled" text were likewise critically inspected while listening to the record (to which they were true) yet twice again, 38% of the consonantal sounds were challenged, and 95% of the challenged sounds which were identified, were identified as the corresponding sounds of the English text. Half of the challenged consonantal sounds were heard as other sounds. Finally, selected portions of the dictaphonic record when heard while both the English and the "garbled" texts, in interlinear arrangement, were before the eyes of the reagent and were alternately critically examined, gave clearly first the one and then the other; that is, *precisely the same auditory stimuli were identified by the same reagent now as English text and again as "garbled" text.*

From the experiments with nonsense-syllables on the dictaphonic record we learned that the mind (versus the ear) supplied 50% of the consonantal sounds as distributed in English speech; the "garbled" text on the dictaphonic record forced 50% of changed consonantal sounds in the English text into the transcriptions. In both of these cases no suggestion beyond what the sounds on the records themselves offered was operative. When the typed English text, however, was carefully watched while the sounds on the record were critically examined, less than 10% of the changed sounds in the English text were challenged; *90% came true to the English text,—were heard as other sounds.* This shows the force of suggestion from the visual text when its influence was toward the false cognition of sounds,—false cognition was increased about 80%. This force was not overcome entirely by the suggestion offered by the typed "garbled" text, since 38% of these sounds were challenged and half of the challenged sounds were definitely identified as other sounds,—almost uniformly as the sounds in the English text. If 50% of the sounds are sufficiently indefinite to fail the ear, the suggestive force of the "garbled" text was 12%; and its influence was toward right cognition.

It is therefore clear that under the conditions of communication obtaining in our experiments a large part of the perception of English words is contributed by the mind and that suggestion is a very potent determinant of the language heard. These facts are not sufficiently recognized in psychical research, although they are of course entirely consistent with psychological knowledge.

The perception of a word seems to the recipient a purely auditory

affair; yet the fact is that a more or less slender, sometimes a wholly inadequate, auditory impression has been assimilated to psychical factors (memory images, attitudes, motor dispositions, and other residuals of experience) which are primarily responsible for the cognition of the word, but which cannot by introspection be segregated from the sensory component of the perception. This assimilating process under the influence of suggestion leads to false cognition,—to illusion. A word is heard, both auditory and psychical components of the perception are referred to the ear, critical examination of the auditory impression is again made, and the word is verified—yet the word is wrong! The sounds falsely identified were assimilated to the wrong psychical factors.[22]

The case of auditory perception is not unique, for, as was intimated in the INTRODUCTION to this PART, all sense-perceptions involve the psychical component to which the bare sensations are assimilated, and consequently all special senses are capable of reporting fact that is not there. A simple illustration from vision may help the reader to establish this principle to his own satisfaction. He, however, is likely to miss the full force of the experience unless he stops reading with the end of this sentence and hands the book over to someone else to get the instruction and carry out the experiment, for the psychical component can only be found by setting a trap for it. [*Stop here and hand the book over.*] (1) Show the word in Fig. 6 to the observer at a distance of about six feet, (2) ask him to look at it carefully, (3) close the book after he has had one "good look," and (4) ask him to draw or trace with care on paper one of the letters (for example I or T or O) just as he saw it. Now, he likely suspects a trick, and he may be able to avoid tracing in the outline that is not there, but the chances are against him. Professor Jastrow [23] found that out of thirty students who were given the same test with similar lettering only three or four drew the letters correctly; and our students commonly see the complete outline of the letter. Even after one knows the form of the letters he can, if he observes in a detached mood, and views the word at the proper distance or through narrowed eye-lids, see the letters raised from their background and lighter in color, the omitted boundary appearing distinctly. Under proper conditions the sensory impression is assimilated to the psychical factors which preserve our experience with outline-letters and the missing outline thus mentally supplied is credited to the eye.

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[22] *Vide*, Introspection No. 5, p. 399, *supra*.

[23] Jastrow: Fact and Fable in Psychology. New York, c 1900, pp. 281-2.

Professor Jastrow, in a chapter on "The Mind's Eye," [24] says:

It is a commonplace taught from nursery to university that we see with our eyes, hear with our ears, and feel with our fingers. This is the truth, but not the whole truth. Indispensable as are the sense organs in gaining an acquaintance with the world in which we live, yet they alone do not determine how extensive or how accurate that acquaintance shall be. There is a mind behind the eye and the ear and the finger-tips which guides them in gathering information, and gives value and order to the exercise of the senses. This is particularly true of vision,—the most intellectual of all the senses, the one in which mere acuteness of the sense organ counts least and the training in observation counts most.

And after he has shown a series of diagrams illustrating visual illusions and reversible perspective, he continues:

This collection of diagrams serves to illustrate the principle that when the objective features are ambiguous, we see one thing or another according to the impression that is in the mind's eye; what the objective factors lack in definiteness the subjective ones supply, while familiarity, prepossession, as well as other circumstances influence the result. These illustrations show conclusively that seeing is not wholly an objective matter depending upon what there is to be seen, but is very considerably a subjective matter, depending upon the eye that sees. . . . Not only when the sense-impressions are ambiguous or defective, but when they are vague—when the light is dim or the forms obscure—does the mind's eye eke out the imperfections of physical vision. (p. 294).

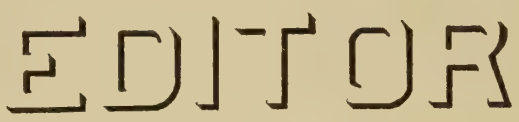

Fig. 6. These letters, if observed from a distance of about six feet, or through narrowed eye-lids, stand out from their background, showing mental contribution of the missing contour,—a mental element in the visual perception of the letters which seems wholly sensory.

The following quotations, selected at random, will serve to show that the foregoing discussion is consistent with psychological doctrine:

Bastian, a professor of medicine in University College, London, who was interested in a psychological analysis of the speech function, said: [25]

Thus it happens that an object is recognized immediately or intuitively, not so much by the mere . . . impression present, as by the blending of this with more or less fully revived memories of other impressions which have at various times been associated with the same object. . . .

It is therefore by the simultaneous consciousness and fusion, as it were, of the subjective side of various new and old impressions that a present object is perceived and recognized.

[24] *Ibid.*, pp. 275 ff.

[25] H. Charlton Bastian: A Treatise on Aphasia and Other Speech Defects. London, 1898, p. 27.

In the same place he quotes from Bain:

When we see, hear, touch, or move, what comes before us is really more contributed by the mind itself than by the present object.

Höffding, professor at the University of Copenhagen, said:[26]

In such immediate and involuntary recognition consists *perception*. The psychological process which here takes place may be described as the fusing of a reproduction and an actual sensation. The percept is thus conceived as compounded out of a representation and a sensation.

James, professor of psychology in Harvard University, said:[27]

Whilst part of what we perceive comes through our senses from the object before us, another part (and it may be the larger part) always comes out of our own mind. . . .

In the ordinary hearing of speech half the words we seem to hear are supplied out of our own head.

Wundt, professor of psychology at the University of Leipzig, said:[28]

This type of association [the connections of an externally excited sense-perception with its related memory-images] we will call *assimilation*, and speak of the memory image as the assimilating element, the sensations following from the sense-impression as the assimilated. These expressions imply that the memorial constituents are the determining factors in the result, while the incoming sense-impressions are determined by them. This is so far true. An impression may be apprehended in the most different ways, according to the disposition in which the mind has been left by previous experiences. The resultant complex idea is, therefore, a mixed product of the impressions given in perception and of an unknown number of memory-images. But, just because the idea is a single complex, there can be no question of analysis into these two constituents. Hence the reproductive elements are invariably referred to the sense-perception, which now contains constituents not to be found in the impression which aroused it. On the other hand, real constituents of the sense-impression may be wanting in the resultant idea, owing to their conflict with reproductive elements of greater intensity. . . .

Lastly, in all these processes of assimilation, which follow directly upon sense-impressions, the peripherally excited sensations are so far of influence upon the memorial elements that they increase the intensity of the reproduced sensations. That is the only possible explanation of the fact that even in the normal assimilation it is impossible to distinguish between the ideational elements aroused by external stimulus and those excited by association. The impossibility becomes still clearer when the elements of the latter kind obtain so exclusive a predominance that the resultant idea is wholly inadequate to the sense-perception. Assimilations

[26] Harald Höffding: Outlines of Psychology. London, 1891, p. 124.

[27] William James: The Principles of Psychology, vol. II, New York, c 1890, pp. 103 and 97, respectively; or Briefer Course, pp. 329, 323.

[28] Wundt: Lectures on Human and Animal Psychology. London, 1912, pp. 287, 289.

of this class we term *illusions*. In the illusion, we imagine that we perceive something which is not there; that is to say, we confuse memorial elements with sense-impressions.

Titchener, professor of psychology at Cornell University, said:[29]

The simplest kind of perception, then,—what we may call the pure perception,—implies the grouping of sensations under the laws of attention. But it is clear that perceptions are, as a rule, not made up solely of sensations; we see and hear and feel more than is presented to eye and ear and skin; the given sensations are supplemented by images. Most of our perceptions are mixed perceptions, complexes of sensory and imaginal elements; and the life of perception is, far more than one is apt to suppose, a life of imagination.

Ladd and Woodworth, in the enumeration of the essential features of the theory of visual perception which they advocate and credit to Lotze, Helmholtz, and Wundt, say:[30]

Fourth: The incalculable, but enormous influence on sense-experience which lies back of all the phenomena obtained for scientific treatment, whether from the physiological, the psycho-experimental, or the purely introspective point of view, must never be lost out of account. Indeed, these *residua* of past experiences, if we may so call them, are doubtless in many cases the determining causes of the character of the new experience. They consist of obscure and scarcely recognized sensations, images of previous sensations, motor tendencies and impressions, fusions of unanalyzable elements, flighty and flitting syntheses that have scarcely the quality of even an instinctively formed judgment; and—perhaps, above all,—workings of the organism which do not result in any effect that rises "above the threshold" of consciousness. But it is just such unrecognized, and largely unanalyzable, factors as these, which chiefly determine, not only our conduct under the direction of sight, but also our seemingly most logical conceptions and deliberate judgments concerning visual objects.

Of course these statements are not mere opinions; they are generalizations from a great mass of experimental results. With respect to perception of words two researches may be mentioned:

(1) Pillsbury presented mutilated words on a screen in a dark-room to be read by his reagents, and recorded as seen. He found that[31]

In many cases it was noticed that the letters which were most certain and of whose presence the subject is most confident were not on the slide, but were added subjectively. Occasionally no word is seen, but only detached letters or a nonsense syllable, which is made up partly of the word on the slide and partly of letters from the word whose presence is due to the disturbing influence. These facts show

[29] Titchener: A Text-Book of Psychology. New York, 1911, p. 364.

[30] Ladd and Woodworth: Elements of Physiological Psychology. New York, 1915, p. 458.

[31] Walter Bowers Pillsbury: A study in apperception. *American Journal of Psychology*, 1897, 8:362, 267.

that for the individual the centrally excited sensations [psychical factors] are just as truly real parts of the word perceived as the peripherally excited [sensations]. . . .

The perception, then, is a very complex process, that can be regarded as the resultant of the stimulation of the moment and of all past experience; as the product of the reaction of character upon the present external forces.

And Bagley, who presented mutilated words to the reagent by means of the dictaphonic record, found that [32]

The consciousness concomitant with the apperception of auditory symbols is made up of sensational and affective elements—some peripherally, some centrally aroused—in connections which vary in character with different individuals and under different conditions.

Our results are therefore in accord with both psychological doctrine and the results of researches on the perception of language. It is safe to conclude that owing to the dominant rôle played by the psychical component in the perception of language, the ear cannot be trusted to report correctly names or phrases, when the latter are spoken under such conditions as are deemed by the recipient satisfactory for communication yet which permit some degree of indistinctness such as is usual when "trumpet" or "independent" voices speak in a séance-room. This applies also to speech and song in a hall, to foreign as well as native tongues, and to the method of preserving speech in an unknown tongue on a dictaphonic record for inspection in search of foreign words and phrases.

If supernormal capacity for the acquisition of knowledge makes possible the naming of friends unknown to the "medium," or the reproduction of knowledge in a discarnate consciousness, as in "messages," which are accepted as evidences of the presence of a discarnate personality, the fact is so important that it should not be allowed to rest upon questionable evidence. The service of this PART should be to make clear the questionable nature of some kinds of evidence—those that do not exclude, but rather invite, an auditory illusion which results from the usually unrecognized psychological process of the "assimilation of sounds in the perception of speech." This would seem to be a necessary step in the search for trustworthy evidence, and, consequently should be acceptable to all who are interested either in establishing the alleged fact, or in the advance of knowledge in psychical research.

For the purpose of establishing a foreign tongue (1) it should be expressed in legible symbols (preferably typewriting) which because of their permanent character may be inspected not only repeatedly but by

[32] William Chandler Bagley: The apperception of the spoken sentence, a study in the psychology of language. *Idem*, 1900, 12: 125.

any competent linguist; (2) it should be recorded under circumstances which meet the requirements of a scientific experiment, thus excluding the production of the text by fraudulent means; (3) a copy of it dated and signed by competent observers, should be immediately filed with a responsible authority (preferably an accredited society for psychical research); and (4) the results of all investigations should ultimately be published in an authoritative journal or proceedings.

For the purpose of establishing a supernormal capacity for the acquisition of knowledge (whatever its nature), the experiments with playing-cards [33] and the application of mathematical formulæ derived from the theory of probability, provide a method already described as safe and sure.[34]

[33] *Vide*, pp. 48 ff., *supra*.

[34] *Vide*, pp. 313-4, 366, *supra*.

PART V.

CONTRIBUTIONS
BY
PROFESSOR LILLIEN J. MARTIN

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A CASE OF PSEUDO-PROPHECY.[1]

By Professor Lillien J. Martin.

At the time of the great earthquake of 1906 the accompanying poster of the Junior Farce PKWTNOPIU of the Class of 1903, that is, a poster made three years before the earthquake, was referred to as a prophecy by some of the newspapers of San Francisco. As it seemed to me of some interest to ascertain whether this was actually the case I wrote to Mr. Johnson, a consulting geologist in Los Angeles, the maker of the poster, in regard to the matter. His letter which I append shows that the contents of the poster[2] grew out of the drawing of an inference regarding the future from the past—is a case, in short, of scientific prediction and not of prophecy.

Mr. Johnson says in his letter of August 11, 1913:

"Mr. Fletcher Wagner, whom you may remember as winner in the Carnot Debate several years ago, first suggested to me the idea of a class play which should be like the famous breakfast food, 'something different.' At that time I was full of the geologic phenomena associated with the earthquake rift which passes southeastward along the San Francisco peninsula, back of Los Gatos, and so toward the Pajaro River. I have seen the effects of crustal movement during past ages along this fault line and have been deeply impressed with the topographic changes which have taken place in this part of California within comparatively recent time (geologically speaking). All of this was, of course, as you know, before the earthquake of 1906, but I realized that the fault line had been the theatre of earthquake activities on a grand scale for a tremendous period of time.

"What better than that Mr. Wagner, with his eye for the dramatic, should see in this geologic fairy tale a chance for a play that ought to appeal to at least Dr. Branner's students. Hence we sate ourselves down and began this immortal work. Fletcher composed the music, words, songs and pretty much everything

[1] Reprinted by courtesy of *The Psychological Review,* from vol. 23, 1916, pp. 163-4.

[2] See the Plate facing page 412 which gives a reproduction of the poster and of a photograph of Memorial Arch which was taken just after the earthquake.

else, so my creative outburst expressed itself, so to speak, volcanically. I knew that the earthquake rift ran northwest and southeast and felt that any high structures near it would probably be toppled over in case of an earthquake. The highest structure at the University, except the chimney, was the arch, and that seemed to lend itself best to a poster. I wanted the poster to be graphic, rather smashing in its effects, if you will, and so pulled out one side of it and left the arch overhanging in an impossible manner, (I hope the engineering profession will have forgiven me this by now). Otherwise I tried to show what I thought would really happen in case the fault line had a chill. So far as any premonitions were concerned I know there were none. I merely drew a poster as best I could on the evidence of geologic facts gathered in the field. As I recollect, the poster was discussed considerably at the time of the earthquake in some of the San Francisco newspapers and, of course, the usual vivid imagination of our newspaper friends called forth a long tale of the astounding clairvoyance of a Stanford Student. Bosh and Rot. You now have the real inwardness of this remarkable event put on paper for the first time."

Fig. *a*.—Poster of the Memorial Arch made in 1903, and a photograph of it taken after the earthquake in 1906.
(*Vide*, p. 411.)

LOCAL GHOSTS AND THE PROJECTION OF VISUAL IMAGES.[1]

By Professor Lillien J. Martin.

Some tests recently made with three Stanford students who reported that they had seen apparitions show most clearly that whether we will see ghosts at all and what their appearance will be depends upon whether we normally project our visual images into space or are able to do so under the influence of a strong emotional stimulus. I give first, in the following, what the students gave to protocol in regard to the apparitions they had seen, and then briefly summarize the results of the tests I have made with them regarding their ability to project visual and other images.

I. Mr. M., a special student in Chemistry.

Time of appearance of the apparition—the first Sunday afternoon in May, 1908. Quite normal in health. I was reading with much interest in Mendeléeff's "Principles of Chemistry." That I was awake I am quite sure, as my sister had just passed through the room and we had spoken. Quite suddenly the book seemed to disappear, and in its place came the appearance of a man, head and shoulders. He wore a battered sombrero and soft dark shirt, open to the second button. About him was the seeming of brilliant sunlight and air of crystalline purity, and the sense of high places and much living out-of-doors. Though I could not see it, I knew that he carried a gun, probably a rifle, under his right arm, possibly suggested by the set of the shoulder. The face was attractive at first glance, rather handsome—or easily might have been. The skin was clear and beautifully bronzed. He looked back at me with a clear, open, frank gaze, with a half-smile on his face. But about him there was a swagger, a braggadocio, an insistence upon his own importance and his will to dominate without much consideration for the other fellow, that, though it was not consciously emphasized on his part, irritated me and left me somewhat ill at ease. He remained quite two minutes, while we stared at each other. The smile, somewhat superciliously amused, grew broader, and then slowly he faded away and the book blurred back into place. I finished the sentence and the paragraph.

Time, June 2d, 1908, about midnight. I had been out to call, had particularly enjoyed the evening and had stayed late. I was sitting on the edge of my bed unlacing my shoes. Suddenly I sensed another personality present and looked

[1] The first part of this article is reprinted, by permission, from the *American Journal of Psychology*, 1915, 26:251-7. For a closely related contribution, *vide*, Martin: Die Projektionsmethode und die Lokalization visueller und anderer Vorstellungsbilder. *Zeit. f. Psych.*, Bd. 61 (1912), p. 321, and published in book form by Barth, Leipzig.

up quickly in surprise and inquiry. The same man stood across the room from me, perhaps ten feet away. The entire figure was visible. He was dressed much as before, but without the hat or the gun. If before his self-assurance had irritated me, there was now about him a conscious, purposeful, insistent masterfulness that I would not and could not endure. It aroused an instant and fiery antagonism. My feeling was—though I did not say it—"You get out of here, quick!" Yet I had a decided impression that he wanted something that I might give. But he did not ask, but demanded, in impossible ways. It seemed, too, that his manner was a pose, assumed as much to impose upon himself as upon me. A bit of unacknowledged fear lurked somewhere back in his consciousness. All this, somehow, I knew. He stared at me a moment, with his insolently irritating smile, took three steps to the side and diagonally toward me, and suddenly disappeared. The incident left me so irritated that I slept little. Early in the morning of June 3rd I left for the "High Sierra" on a two months' camping trip.

About three weeks later, when we reached the upper canyon of the Kern River, I had a very strong and quite unreasonable, persistent desire to go farther up the canyon, which was not possible at the time. This desire, after a day or so, associated itself with a strong sense of the personality of the man described above though I saw nothing. So compelling became the desire under the stimulus of this association that one night, somewhat after midnight, I walked almost without volition of my own, perhaps six miles up the trail in the moonlight until an impassable stream halted me. I seemed to *know* that this man was there up the canyon, and because he was there was I compelled to go.

We camped, three days after this experience, at the edge of Monachi Meadows. About midnight I was awakened, very suddenly, by someone shaking me roughly by the shoulder. I sat up. The same man stood beside me, hatless, but with his gun. I saw the glint of the moonlight on the gun barrel when he moved. He seemed to be in overwhelming terror. The qualities in him that had so irritated me before were quite gone,—the braggart was become the craven. I knew, somehow, that he came as a supplicant. He said nothing, nor did I. After staring at me a moment, his head drooped in sudden hopelessness, and he walked away.

On the first occasion I could not see the book through the man just described. The second time, he was transparent, so that a chiffonier back of him was quite visible. So, too, the third time, I saw the trunk of a tree through him. That is, he was opaque the first time, but semi-transparent on the two other occasions.

In 1901, in connection with a philosophy course, given by Dr. Lovejoy, I read much Hindu philosophy; and I remember that Dr. Lovejoy talked in his lectures of Egyptian religious beliefs, among others. I was much troubled that spring by a series of apparitions. I do not remember just when or under what circumstances these apparitions commenced, nor how many times they occurred, but they occurred very frequently. The details of each occasion of their appearance, what they did and said, I do not recall. One frequent astral visitor was a Hindu, seemingly a pundit or yogin; another, less frequent, was an ancient Egyptian, whom I much disliked. Upon the appearing of these apparitions I would be first conscious of the eyes, from which point the rest of the figures would slowly develop downward. Their disappearing was the reverse process, from the feet to the eyes, which would persist sometimes much longer, at times for hours. The figures

always were semi-transparent, though very distinct, so that objects were quite visible through them. They frequently talked to me. The Hindu, particularly, would deliver long discourses to me which at the time seemed the profoundest of wisdom, but which, upon later analysis, I always found to be a vague jumble of meaningless phrases. At first I permitted these apparitions because they amused me, and I could banish them at will. But later they became more persistent and I could not rid myself of them. Particularly the eyes would appear and follow me, without the rest of the figure becoming visible, which caused me much annoyance and distress. I remember quite distinctly the last occasion on which I saw them. I was alone in the country. Upon arising in the morning about 7:30 I went to the piano to play. Suddenly I saw the eyes of the Hindu above the piano. Of what happened thereafter I know nothing. I found myself at 9:30 in the evening exceedingly weary, still sitting upright at the piano. I do not know whether this was normal sleep or of a hypnotic nature. I had had some slight experience with auto-hypnosis, having on several occasions put myself to sleep by looking at a bright object, first resolving to awake in five or ten minutes.[2] I was so frightened by this experience at the piano that I thereupon determined never to see these particular apparitions, or their eyes, again, nor to go to sleep without my own volition, nor have I. I have, however, occasionally seen other eyes. For instance, a few evenings ago I was reading in bed when a single eye, about four inches long, appeared back and at the right side of my book. I glanced at it and continued my reading. Presently the eye came nearer and slid over the edge of the book. It was transparent, and the words were quite visible through it. It persisted about ten minutes. In general in looking at persons I am quite conscious of their eyes, as the eyes are to me the most noticeable and important feature of the face; just as the head is of greater importance than the rest of the figure to me. In looking back at this whole experience I see clearly that it is all to be explained by the projection of visual and other images, perhaps with auto-hypnosis as a subordinate factor.

As a child, from my earliest recollections to the age of about twelve, I had a considerable number of imaginary playmates who seemed very real to me and of whom I was very fond, so that I cared little to associate with other children. To each of them I gave a name, and each was a distinct and well characterized personality. When they finally ceased one by one to come to see me I often regretted their absence and wondered what had become of them. It was, therefore, a matter of great surprise, in 19~5, to meet on the street in Palo Alto, a young man (Dr. A. L. Münger, Jr.), whom at once I recognized as Futoni, one of those friends of my childhood. The fancy still persists that the two are identical. And I have since met two other men who just as surely are identical with my imaginary friends. These experiences of my childhood made me immediately accept as self-evident the theosophical idea of reincarnation when it was by chance presented to me in later years. That this particular group of apparitions were

[2] We see from this experience of Mr. M. that with him as a reagent we could doubtless easily have developed, artificially, at that time, a state of "Trance," and thus have been able to study the origin and evolution of a "medium." It would not have been necessary at the time of this occurrence to build up the "controls" by means of suggestion for they were already at hand in the persons of the Egyptian and Hindu.

projected visual and other images, I am satisfied. As to their character, I am still in doubt, for my early environment does not seem to me to adequately account for their content.

I add still one more of Mr. M.'s projected visual images:

After spending the afternoon of Wednesday, March 18, 1914, in the petrography laboratory working at the same microscope with Mr. H., I called upon Mr. W. in Palo Alto. As Mr. W. and I were walking to the street-car about six o'clock, I saw a transparent image of Mr. H. walking just in front of Mr. W. on my left. When I left Mr. W. the image of Mr. H. followed me aboard the car, sat by my side, and went home with me. I confess I had a strong impulse formally to present him to my wife. The image sat in a chair in the living room when we went in to dinner. He was in the same chair after dinner, remaining with the family for half an hour. When I went upstairs to read the image followed me and remained until nine o'clock, when he walked out of the room.

Tests with Mr. M. in the laboratory, show: (1) That his visual images are strong and that when he casually thinks of an object such images are located in the back of his head, but if he thinks of the object in detail the image of it is out in front of him, the distance away depending upon the circumstances under which the image is seen. (2) When he projects visual images of persons he has seen, they are translucent. He does not usually see through such images but can easily do so. The transparency of his hunter, etc., images, was doubtless due to the presence of the bright light which was so emphasized as to divide the attention as, for example, in the case of seeing the "glint" of the moonlight on the gun. (3) When he thinks in the laboratory in detail of the hunter, the projected visual image which appears has the same general appearance as the corresponding apparition except that it is slightly less transparent. The projected image in the laboratory seems "hardly as real" as his apparition, however. He feels it is an image of his apparition, that is, that it seems to have the same relation to it as does the projected image of an object. The emotional experience with its kinaesthetic accompaniments localized largely in the back of his neck and arms (he has a very slight lesion in the back of his neck which produces, at times, a feeling of numbness and a dull ache in his arms), which are elements of the feeling of reality, is there but it is much decreased in strength. This hallucinatory experience as well as others like it show that it is not alone necessary to be able to project one's visual images and to do it usually or very frequently in order to have a hallucination. Mr. M. does on occasions when he has no hallucinations. Other persons who have strong visual images and usually project them have never seen an apparition. The hallucination image

must have a content or be accompanied by an emotion which separates it out from other projected visual images. (4) Mr. M.'s auditory images are projected. When he thinks of a sound he hears it again and it seems to come from where it originally came. (5) He can project touch images very imperfectly.

II. Miss T., a student in the German Department, graduated at Stanford University in December, 1913. Early in December I was told that Miss T. had seen a ghost. I give below a curtailed account of the interviews I had with her concerning the apparition she had seen.

December 8, 1913, evening. My own study. Miss T. said:—"I was awakened with a start on the night of December 5th, at 2:15 a.m. When I opened my eyes I saw my ghost, which I had often seen before, standing in my window. He has a high forehead, very hollow eyes, which appear to be only black blotches. His eyebrows are very dark; nose large; very sunken cheeks, and a very prominent chin. He wears a long white flowing robe. His hands are thin and bony. I lay in bed and looked at him. He did not move. He had a long chain in his hand. My bed is next to the window, so by putting out my hand I could have touched him. Finally I sat up. When I did so, he vanished out on the porch (which is off of my room) and sat on a couch out there. I got up and turned on my light and I could still see him, just as plain as when it was dark. Then I took my eyes off him and looked in the mirror to see if I were awake. Then I looked back and my ghost was still sitting in the same position. I crossed my room and turned on another light. He followed me, keeping about three feet behind me. I looked for the feet of the ghost but could not see them, on account of the long robes. Then I went out in the hall into another room. He followed. While I was in this room he stood in the open door. When I came out he stepped out of the way and allowed me to pass. He followed me until I reached my door and then vanished."

"Were you afraid of him?" "No. He fascinates me. I never want him to leave"

"Why not?" "I have always felt he would bring me good luck."

"Why did he come at this time?" "I do not know. But about five days before, our housemother had remarked that it was about time for my ghost to appear."

"Did he speak?" "No."

"How do you explain this ghost?" "I have always explained this as an hallucination, yet I have a feeling that it is a spirit and have always felt this. I feel that it comes from another world, of which we know nothing."

"What do you mean by an hallucination?" "It is a visual image so firmly fixed in one's mind, that it appears without any conscious effort on the part of the person."

"Did the ghost bring any message?" "No."

"Has it ever brought a message?" "No."

Room was darkened. "Can you see the ghost now?" "No. But I saw a large rectangle of white."

Second trial. "Even feeling that it would be a good omen, didn't make it

come." (She has just written me that she has after many trials succeeded in getting a visual image of the ghost but only with closed eyes.)

"Give an account of previous experiences with this apparition." "It first appeared when I was about twelve years old. It was my idea of a ghost, from stories. It has appeared since on the average of twice a year. The apparition always appears in the window and grows more distinct each time. The other night it was closer to me than it ever had been, except in one instance. On this occasion, it came and sat on my bed, and was about to speak to me, when I called my roommate. The room was dark. The minute I spoke it vanished. I am sorry I called because I am sure it would have spoken to me. This ghost is always the same in appearance but always carries something different. I remember that once it had a large red book, another time an umbrella, and another time a key."

"Have you ever had any other similar experiences?" "No."

"Did you read ghost stories as a child?" "Only a few. My mother did not approve of my reading them."

"Was there anyone who encouraged you to read them?" "My aunt occasionally told me ghost stories."

"Is there any one in your family who believes in ghosts?" "No."

"What is your physical and mental condition in general when the ghost appears?" "It comes at times when I am physically well, happy, and not mentally tired."

Tests:—Instructed her to recall in turn, Dr. Jordan, Miss McCracken, etc., and to see them out in the room before her. She reported that she saw each person distinctly.

"Do they appear solid?" "Yes."

"Can you see Mrs. Hurd?" "No, I don't know her very well."

"Can you put a chair at the side of this real chair?" "Yes: It is almost as real as the chair itself."

Pointing at a reproduction of Raphael's Sibyls, hanging on the wall, "Can you place at the side of that picture another like it?" " In general composition, yes, but not in detail." Repeated with Böcklin's Selbstportrait. "I can get Böcklin and the frame and mat clearly. The frame, however, is not as clear as the chair was."'

Miss T. remarked during this visit that she was going to question the ghost next time. I suspect it will reply for trial showed that she has the ability to project auditory images. Her images of touch are also projectable. She says this ghost is a proof or confirmation of what all her reading has more and more convinced her, namely, that there must be another world. Recently Miss T. has sent me some notes found in her diary in regard to the ghost. February 2, 1901:—"My dear old ghost was here last night. He had a Latin Grammar with him. I hope he doesn't have to struggle as I do with the subjunctive." July 15, 1901:—"The idea of carrying an umbrella in the middle of summer! That's all the sense my ghost has. He came bouncing in with an umbrella last night. He closed it as he entered through my window. He was so busy seeing if he could find any holes in my stockings, that he forgot to take his umbrella with him when he left. I couldn't find it any place in my room this morning, though I looked everywhere. I guess he came back and got it when I was asleep." January 9, 1902:—"My old ghost amuses me so. Last night he did a fancy dance at my window. Then he came and rested in my rocking chair. The nerve of him! As soon as I spoke he vanished."

In my office two days after the tests mentioned above when I repeated some of them with like results. Miss T. gave me her reasons for not believing the ghost is merely a projected visual image:—1. The fact that the ghost came when she was well made her feel it was a real ghost for she supposed projected images came when one was tired or not well. 2. The fact that she was not afraid of it also made her feel it was real. 3. She cannot project her ghost at will or make it come at night when she wishes. 4. She said, after I had tested her regarding her ability to project her visual images, that she had thought in the past that the ghost was more real than a projected visual image but recent trial with projecting the image of a very intimate friend had convinced her that the ghost was no more real than the image of the friend. She added that neither the ghost nor her friend's image follows her eyes in turning her head. The ghost, she also said, followed her when she walked away but her friend did not and then added, "she is very deliberate."

It will be seen from what has been just said, that Miss T. is ordinarily able not alone to project her visual but also her auditory and tactile images and yet she is not able to project her ghost at will. This fact shows that the ability to project one's images is but one of the factors involved in the seeing of ghosts. In the case of this ghost there is not only a favoring emotional factor which is connected with its appearing but one of an inhibitory character which interferes with its arising except on propitious occasions. The favoring and inhibiting emotional complex which has prevented a very lively visual image of childhood from disappearing is doubtless of a religious nature. The ghost coming, as its seer believes, from another world, is a source of intellectual comfort. Through its presence she has been enabled to successfully resist that skepticism regarding the existence of another world which she thinks might otherwise have been engendered by her University studies.

III. Mr. ———, a student in the Department of Philosophy.

Lack of space prevents me from giving a detailed account of the numerous apparitions seen by Mr. ———. One observation I wish, however, to draw attention to as it is another confirmation of my opinion that ghosts arise only where one is able to project his images. Mr. ——— is able without difficulty to project his apparitions into space but he tells me that the images of his apparitions and the apparitions themselves communicate with him largely through signs. He very rarely hears them speak. Their preference for the gesture-language was readily understood when it was found that all of his auditory images were very weak and that he is able only with great difficulty, if at all, to project them into space.

IV. Mr. B., a student in the Department of Physiology, writes:

My nervous system received such a shock, at the death of my wife three years ago, that I could not sleep at night and listened to every noise in my neighborhood. I began soon to hear a voice during the night, accusing me, that I had

not saved my dear wife from her dreadful disease—progressive paralysis. At first I thought that some mischievous person had put a megaphone on the top of the mountain, not far from the house, and I went early in the morning in search of it, but the voice accompanied me, and I soon found out that it was my own brain that invented it. As soon as I came to this conviction, I thought, if I would change the subject, I must hear a corresponding change of this voice. I thought of travel and immediately I heard: "He is going to travel," etc., repeated three or four times. No matter what I thought of, I could hear it from outside. Now it was clear to me that I heard my own thought. I visited San Francisco and while there consulted Dr. Pischel, who could find absolutely no difficulty in either of my ears. The change to San Francisco seemed to have a beneficial influence on my hallucination, and today I hear only a very slight ringing in my right ear, which was brought on 15 years ago in consequence of a cold.

Trial with Mr. B. shows that he has auditory images but is able to project them only imperfectly.

For several years I have given as a class exercise the following questionary sent out by the English Society for Psychical Research:—"Have you ever, when believing yourself to be completely awake, had a vivid impression of seeing or being touched by a living being or inanimate object, or of hearing a voice; which impression, so far as you could discover, was not due to any external physical cause?"

The following is a tabulation of the data thus collected regarding the number and kind of the hallucinations which the students have had:

Hallucinations	Report of Stanford Students.					
	1912	1913	1914	1915	1915	Total
Visual	2	12	6	4	9	33
Auditory	37	37	23	25	33	155
Tactile	6	7	7	6	0	26
Visual and auditory	..	..	1	16	1	18
" " tactile	..	..	..	2	1	3
Auditory and tactile	2	6	..	5	1	14
Visual, auditory and tactile	..	..	..	6	1	7
No. having hallucinations	47	62	37	64	46	256
" " no "	53	58	75	42	107	335
Total No. persons questioned	100	120	112	106	153	591

A comparison of these reports with that of the English Society[3] will show that the proportional number of persons who have had hallucinations is much larger in the reports of the Stanford students. Also, the proportional number of auditory hallucinations as compared with the visual do not agree in the two reports. In the English Report, and

[3] *Proceedings S. P. R.*, 10:25, and Parish: Hallucinations and Illusions, 83 ff.

the same is true in Vaschide's [4] report, the number of visual hallucinations is much greater than of auditory. In the students' reports on the contrary, the auditory hallucinations are much more frequent than are the visual. In view of the agreement of the students' reports in these respects from year to year,[5] I am inclined to believe that such simple auditory hallucinations as the hearing of the ringing of the door bell or that of the telephone when they had not rung, and of the mother's voice when she was present but had not spoken or when she was many miles away, were overlooked in answering the questionary the results of which were tabulated in the English Report, and that auditory hallucinations are probably much more frequent than one would suppose from this report.

Subsequent questioning in connection with the students' reports just mentioned shows that the possibility of having visual hallucinations is not entirely dependent upon the ability of a person to project his visual images in the normal condition. For example, of the twenty students who reported having had visual hallucinations in the last report in the table, four were unable to project their visual images. The abnormal mental condition accompanying the hallucinations, where expectation, fear, etc., doubtless played an important rôle, produced visual image projection.

[4] Les Hallucinations Telépathiques, 56.

[5] Compare Martin: Die Projektions Methode, 128.

AN EXPERIMENTAL STUDY OF THE SUBCONSCIOUS.[1]

By Professor Lillien J. Martin.

The subconscious is so often referred to in works on psychical research that it seems to me a condensed summary of a recent investigation I have made may not be out of place in this report.

In making this study the image method was employed. That is, to state very briefly the mode of procedure, in one half of the experiments the observer, usually with his eyes closed or blindfolded and seated opposite the experimenter, was instructed to sit in a relaxed position and let an image (visual or auditory, memory or imaginative, etc., depending upon what was desired by the experimenter), arise of itself. The observer was not only not to arouse the image but he was not even to know its content until he saw it before him and only those images were noted where the instructions had been entirely complied with. In the other half of the experiments the observer was directed to arouse the image, that is, he was instructed to decide on the particular thing he wished to visualize and to arouse the corresponding image.

Stanford and Münich University students acted as observers.

An examination of the data regarding the content of the images, their mode of arising, etc., shows:

1. The subconscious mental activity reveals itself through the arising of images where the observer did not previously know whether anything would be imaged or if so what it would be. Also, in the arising of unwilled (spontaneous) images in connection with those willed.

2. Evidently, sometimes and in some persons, the subconscious thinking responds more quickly to the task set than does the conscious. This is shown by the spontaneous images arising more promptly than do the willed. That is, the spontaneous image is before the observer before he has decided what image to arouse or arises in place of it.

3. The images show that not only the conscious but the subcon-

[1] For fuller details of the investigation as to the theory underlying it, the methods used, the experimental data, etc., see Martin: Ein experimentelles Beitrag zur Erforschung des Unterbewussten (Barth); and Über die Abhängigkeit visueller Vorstellungsbilder vom Denken, *veit für Psych.*, 70:212. A partial summary of the present report was published in the *Psychological Review*, 1915, 22:251-8.

scious mental activity differs in richness of content in different individuals.

4. In case of all the observers—but in some of them more than in others—some of the material stored away under the threshold has evidently remained as originally grouped, as for example when the visual image of a particular man in a particular environment arises simultaneously and at once. On the other hand some of the material has evidently been more or less broken up, as for example where an eye arises spontaneously when an imagination image of a face is asked and no other features followed it until aroused by a special act of will on the part of the observer. In case of some of the observers the broken up memory material, the memory elements, have been unconsciously (shown by the observer's great surprise at the content of the visual images which arise) recombined under the threshold into complicated and appropriate new groups. There has been not alone a breaking up of memory material but, to use Ribot's words, an "unconscious elaboration" of it. In the observers with whom I have experimented the memory activity evidently predominates below as well as above the threshold of consciousness.

5. The memory and imagination material under the threshold is evidently not all on the same stratum or level of consciousness for some of it arises much more spontaneously and quickly and has a different content. Here too individuality plays a great rôle.

6. From what has been said it will be seen that the visual image method makes it possible to obtain information regarding the past life of the individual, the general character and the personal peculiarities of the thinking going on in his mind, not alone above but also below the threshold of consciousness. The applicability of this method in the case of a particular person will of course depend upon his ability and habit as regards the imaging of his conscious and subconscious thinking.

7. The introspections show that the spontaneous images are sometimes the point of departure of the willed images, that is the involuntary image that arises before the observer has decided what to will acts in the way of suggestion. This shows how important the spontaneous images must be in our daily life. Where the spontaneous images are in the direction of the work in hand, they must save time in that they arise immediately and furnish material already elaborated. On the other hand if they are not of such a character that they can be used directly in the intellectual work being carried on or as points of departure for conscious thinking along the desired line, they must be an interruption and even a hindrance in the continuing of such thinking. The results show that the

spontaneous images may furnish ideals as regards action. In this respect they may or may not be entirely helpful. One of the observers who took part in these experiments has very strong and insistent spontaneous auditory images. So insistent are they that she tells me that they led to her giving up the study of music to which she had devoted several years, and turning to a totally different field of work. She says that whenever she plays on the piano the spontaneous auditory images precede what she is playing and show her how imperfect is her execution.

8. A comparison of the content of the voluntary images with that of the ones which are spontaneous shows that in the case of the visual images of a given observer what is above and below the threshold of consciousness is not materially different.

This result does not support Binet's [2] theory regarding the nature of the subconscious, which is that there are two personalities running side by side, one above and the other below the threshold of consciousness, as what is above and below the threshold of consciousness, as was said, seems in the case of these observers not to be materially different. It may be otherwise in pathological persons of course. Cases of double personality certainly suggest this. But such special cases do not give Binet's theory any great universality. Nor does Myers' [3] theory, which has found support among workers in psychical research, that the subconscious is an expression of the infinite mind and the conscious an individual matter or a very limited expression of the infinite, get support, for as was just said what is under the threshold does not seem enormously richer in content than what is above. Nor do I find anything in these results which leads me to suppose that under the threshold a mental condition exists which makes it necessary to suppose that communication between different persons (telepathy) is possible and which would more or less be supporting Myers' theory. The results do support Prince's [4] theory that what is under the threshold is an expression of the observer's previous experiences.

9. The results have a farther interest from the standpoint of general psychology.

A. They show, that the difference and likeness between spontaneous and voluntary images ought not to be overlooked in psychology, as has been the case in the past, since through the study and comparison of such images we may go below the threshold of consciousness and get some information regarding what is going on there.

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[2] On double consciousness, etc. [3] Human Personality, 1904, I, 34 ff.

[4] The Subconscious, 1914, 1 ff.

B. They throw light on what is called inattention and vacillation of attention. We see that sometimes at least this grows out of the fact that the person has a flood of spontaneous images and ideas, that impede and even crowd out voluntary images and ideas. They explain why the genius is so impatient of restraint and may sometimes actually get on faster by letting himself go and also why the student in a field of a more exacting and foreign character as regards his natural thinking must take himself in hand or fail altogether in his work. I take the following in the way of illustration from what one of the observers gave to protocol.

Als ich mein Studium begann, war es mir kaum möglich, mich in einer Vorlesung irgendwie zu konzentrieren, weil ich beständig durch spontan auftretende V. gestört wurde. Ich habe dann versucht, die spontanen V. zu verdrängen und grosse Mühe darauf verwendet und habe es darin bis zu einer gewissen Fertigkeit gebracht, so dass ich jetzt spontane V. willkürlich haben oder nicht haben kann. Sobald ich mich aber etwas gehen lasse, sind die spont. V. da und ich bin ziemlich machtlos dagegen.

C. The dates obtained lead one to ask whether in future memory investigations along quantitative lines the task of the investigation will not be something more than a filling in of the gap left in the work of an Ebbinghaus and a Müller, something more than a building upon the results already obtained by them. May we not possibly be obliged to begin again at the very bottom and repeat the work in order to feel sure of its foundations? It would seem from these results that instructions given by an experimenter favorable to voluntary effort, or the belief on the part of the observer that he must put forth his will in connection with the task set, while favorable to voluntary memory may have been detrimental to spontaneous and *vice versa.* In short, it does not seem entirely impossible that two persons may have equally good memories as regards the amount that can be reproduced but that like instructions, as for example, that effort (resp. no effort) is to be used in reproducing a given material may make it appear that one person has a much better memory than the other or indeed that neither has a good memory.

D. Again, these results put in question the results of certain experiments of Rux,[5] which were inspired by Ach. Rux has attempted to measure the strength of will by using the quantitative data derived from memory experiments without apparently making any attempt to show how much of the work done was accomplished by voluntary and how much by spontaneous memory.

[5] "Ueber das assoziative Aequivalent der Determination," Untersuchungen zur Psychologie und Philosophie, Bd. II.

10. The results have a pedagogical interest.

A. In that they show that it is possible to educate and enrich the subconscious.

B. In that they lead one to ask whether we may not sometimes be placing too much emphasis on the employment of will in connection with the intellectual work to be done. When the student's work is of a creative nature or along the line of discovery and his spontaneous thinking and images are in harmony with the field in which he is working, one can think that the director of a leading institution in America which is devoted to scientific research showed psychological acumen when he urged the investigators working under him to take each day some time away from their work not only to give their minds rest but to free themselves from the restraint of thinking in one particular narrow line.

The Image Method versus the Automatic Writing and Speaking Methods of Penetrating below the Threshold of Consciousness.

Binet and others have used the automatic writing method in investigating the subconscious. As the image method will naturally come in competition with the automatic writing method in investigations along this line, I have thought it desirable to make some experiments by this method to ascertain how it compares as regards the amount of data yielded with the visual image method in the getting of information of what is going on under the threshold of consciousness.

I cannot refrain before describing these tests from mentioning an interesting case of automatic writing that came under the observation of one of the English Department critics at Stanford University. The critic told me she found written repeatedly on the margin and in the body of a theme handed in to her by one of the women students, the name of a football hero. When shown what she had written, the young woman displayed great embarrassment, acknowledging, however, that she was deeply interested in the young man and often thought about him.

Preliminary orientating experiments, the results of which are given in the note below,[6] were made with Jastrow's automatograph upon 17

[6] *Instructions.* Shut your eyes during experiments 1-2 below. Open your eyes and look at the experimenter during experiments 4-6.

Method. Ten seconds were allowed to elapse after the ready signal in order to have a line for comparison. At the end of 10 seconds the experimenter gave the direction for experiment 1. "Hold your hands still and think of nothing in particular." When 10 seconds more had passed the pencil was raised from the recording paper, that is, the complete experiment lasted 20 seconds.

students to ascertain the probability of any of them being able to write automatically. Involuntary movements of a directive character showed themselves very clearly in case of three of the observers. In case of the other observers, while there were isolated cases of directive movements, the involuntary movements as a whole had a complicated and

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Experiment 2. Hold your hands still and think of Roble Hall.

Experiment 3. Hold your hands still and think of Encina Hall.

Experiment 4. Hold your hands still and watch the experimenter. In first case, a circle was drawn in the air by the experimenter. 5. In the second case a movement was made in the air from right to left. 6. In the third, from left to right.

To avoid any movements growing out of a knowledge of what was to be expected, the above experiments were purposely made but once with each of the observers. Nothing suggesting automatic writing was found among the tracings. Involuntary movements of directions were discernible in the records of all the observers but they were present in every case in the records of but one of them. Some of the records, apparently having no significance, were understandable in the light of what the introspections showed the observer to have been thinking of. Others having a more complicated character were not understandable. The results of the introspections, the observations of the observer and the tracings which I summarize below show the large number of factors entering into such an experiment.

(1) The length of time given for the writing was probably too short for the observer to get to thinking strongly. (2) There was a decided preference, by the observer, for movements in a particular direction. (3) There was a swaying of the body, as a whole, which unsteadiness was increased by closing the eyes in the case of one of the observers. Breathing and other movements may be of a character to inhibit or compensate the arm movement. (4) Observation showed the movements of direction were sometimes of the eye and head and not of the arm. (5) Where the observer had not orientated himself in the particular room as to location of the two buildings mentioned appropriate movements of direction were not generally present. (6) Encina Hall was at the observer's right but on hearing the instruction, some of the observers had a V-image of it as if located directly in front of them, and in others the observer felt himself in the reception hall of this building dancing a waltz, as he had often done. (7) Affective elements enter in as, for example, the observer felt himself being drawn towards a building or repelled from it because he liked or disliked it. (8) Telling the observer not to move his hands not only directs attention to his hands and disturbs their normal movement but evidently had an inhibiting effect and doubtless, in some cases, resulted in movements in the opposite direction. (9) There may be simultaneous lines of thought each of which gives rise to writing, one producing involuntary writing and the other automatic. This may result in a writing very difficult to decipher.

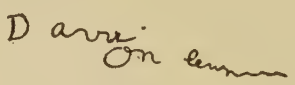
On examining the tracings in the light of the introspections one is again impressed with the futility of depending entirely on the results of "objective psychology." As a control it is of course of value but it is evident again from these results that one can not get very far in the study of the mind with it alone.

confused character. Nothing of an automatic writing nature showed itself.

Subsequently experiments were made with five of the observers above mentioned and with two other persons, which looked directly to the getting of data on automatic writing.

Method. A soft pencil was put in the observer's right hand which rested on a sheet of paper and was screened off so the observer could not see it. An open book (Emmy Lou, by G. M. Martin), was laid on the table at the observer's left and he was told, after hearing the instructions as to what he was to write automatically, not to think about what had been said or of his right hand but to give his entire thought to the reading of the story. The time allowed for each test was five minutes. In order to get material of sufficient amount to be compared with the published reports on automatic writing, it was necessary to give this long period. In M's case there was usually a period of one or two minutes before the writing began. Having been begun it was continued until the end of the period. The slowness of the writing may be explained by supposing consciousness took some part in it. It is explainable, however, in another way, namely, by supposing the writing was under the control of another set of nerves (a lower set) than in ordinary writing. This last supposition would perhaps throw light on the preliminary twitching of the hand observed in the early stages of the writing and the feeling of numbness and "asleepness" of which the observer spoke and of which he tried to get rid at the end of an experiment by rubbing his arm. If this be true, the twitching was an indication of the transition of the control of the hand from the control of the higher to the lower nerve centers that ordinarily controlled the writing, and the "numbness" to the fact that the lower centers, as regards the writing, had assumed control.

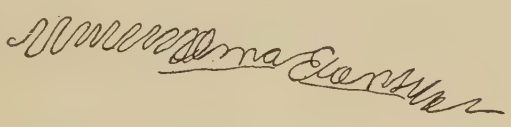
In case of two of the observers there were again involuntary movements but no trace of automatic writing. I give below the writing and the corresponding introspection of one of the three remaining observers, when the instruction to read was given. It will be seen, the writing is largely an involuntary movement and shows that involuntary movement does, without doubt, sometimes play a part in automatic writing:



("Davis on lawn") Knew my hand was moving, but did not know I was writing words. Remembered my first teacher, America Davis, and had a visual image of her in the school-room. No KV or AV images. Recalled consciously indis-

tinctly the time I was first scolded by a teacher for walking on forbidden lawn, and how another teacher Miss Davis comforted me by putting her arms around me because I had not done the forbidden thing knowing it was wrong.

In case of the two other observers, there were also a few incomplete words written. For example, when the instruction was "Write the name of some person whose name you are unable to recall," one wrote: and gave to protocol:



I had to try, consciously, all the time, to keep all my attention on the book, for I knew that something was to take place without my consciousness, and curiosity bothered me a little. In one paragraph the reading was less interesting than usual—my attention wandered—and I heard the name I had evidently been trying to recall. But I have the feeling that no subconscious phenomenon produced the name—I had only succeeded in drawing a curtain between two parts of my consciousness, so that they should each be ignorant of what the other was doing.

Here the introspection shows that what was written was far too near the threshold of consciousness to be called a case of automatic writing. There has been, through the use of the distractor, merely a dividing of consciousness—focal had been separated from marginal experiences.

I believe that in cases about to be given the two remaining observers, M and O, wrote automatically. My use of the word "believe" needs to be explained perhaps. Of the personal integrity of M and O I am absolutely certain from my personal knowledge of them and from the report of people who have known them well for a long time. They have, however, had little psychological training, their laboratory experience being largely confined to these and the previous experiments. It is almost superfluous to remark that introspection extending over a period of five minutes makes very serious demands upon an observer, even of long training. I should perhaps state that I look upon what I report here as truly as automatic in character as that reported by any other of the investigators in this field, Binet, Janet, Prince, etc. Indeed, I may say, I am inclined to put more confidence in these results than in theirs for the following reasons: (1) The observers have had, doubtless, more training in introspection. (2) Their characters are probably more unified and stable. (3) They have both had a university training in science; both are earnest and capable in their university work, O's work being regarded as exceptionally good. (4) They are quite normal,

and (5), judging from what has been reported in connection with automatic writing, I take it that more pains has been taken in these experiments by constantly drawing the observer's attention to the danger of imperfect observation, to guard against it.

The results of the experiments made by M and O follow. In case of each experiment I give on Plate *A*, a tracing of what was written automatically, in a parenthesis the content of the writing as I decipher it, and finally the observer's introspections in connection with what he had written. At the end of each test the observer was questioned as to his knowledge of what he had written. Where nothing is expressly recorded in connection with the test the observer did not know. To farther test the automatic character of what had been written, it was shown to the observer in nearly every case, immediately after the experiment was finished and he had given his introspection to protocol, and he was questioned as to the presence of ideas in consciousness in any way corresponding to it. Results with M:—

Exp. 1. Imagine a landscape. In each experiment the observer was told the imaging and recalling was to be entirely subconscious.

(The Greek letters Delta, Phi, Sigma, Mu and Alpha will be recognized in the writing.) After being shown the writing, the observer said, "Coming out on the street-car I read a mathematical article in which Greek letters were employed in the formulæ."

Exp. 2. Can you recall the color of the dress Mrs. S. wore last night? Answer. "No." Directed the observer to write the color automatically. ("la, la, lave lave") "The question was, 'Do you know the color of the dress Mrs. S. wore last night?" "Felt hand moving with nervous twitching, but had no idea as to whether it was writing or what it was doing. The feeling of the arm was uncomfortable, as though it were 'going to sleep.'[7] When I saw the 'lav' I recalled at once that the dress was lavender."

Exp. 3. Image something. Write automatically a few words telling me what you were imagining under the threshold of consciousness.

("death penalty") "As I read, there came spontaneous visual images,—first of the scaffold at San Quentin; then the warden standing on the scaffold, and a number of men, rather indistinct, moving about; then, of the black cap or hood being drawn over the head of the man to be executed. The image of the black cap became very strong, almost blotting out the page I was reading, and the rest of the images faded. There was a very strong emotional reaction. In July, 1913, I witnessed an execution at San Quentin. The images of the scaffold and of Warden Hoyle were recognized. The rest of the images were imaginary. I was not at all conscious of my arm and hand, or of writing anything."

[7] The ease with which M. followed the instructions as to what he was to write even where he was not conscious of doing so, explains the occasional non-effectiveness of distractors. In some cases, where the distraction does not seem to materially affect the perfection of the work accomplished, the task set is doubtless carried out automatically.

Exp. 4. Image something.

("Three hills red silver Jack Campbell") "I was conscious of some movement in the hand, but had no knowledge of its nature." On seeing the writing, observer said, "Jack Campbell was a mining man whom I knew in Goldfield, Nevada, some years ago. The mountains in this region are often brilliantly colored."

Exp. 5. Remember something.

("Casper Kummer's funeral rain Jack") "Consciousness of movement, not

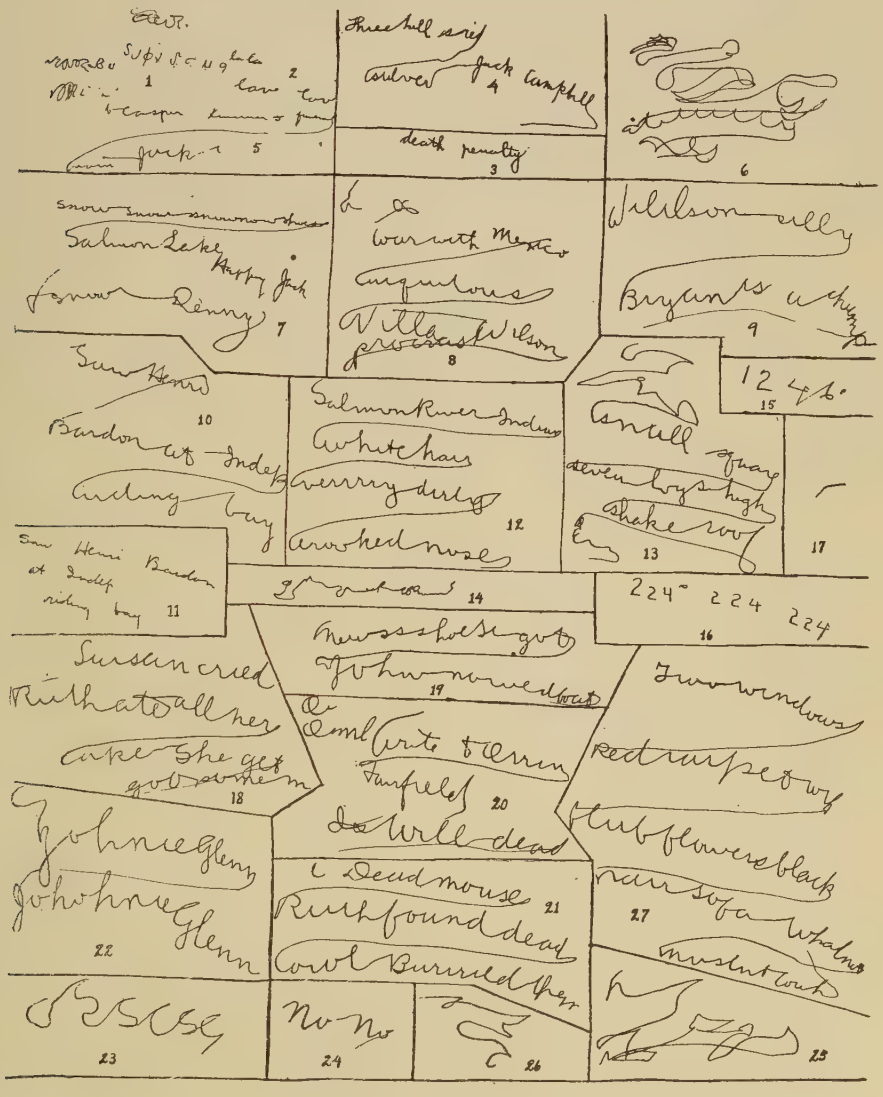


Plate *A*.— Observer M.

of what was written. Not conscious of contractions in hand. I took charge of the funeral of one Casper Kummer, at Goldfield, Nevada. During the service there was a violent thunder storm. I did not recall the occurrence until I had looked at what I had written. I do not remember any 'Jack' in this connection."

Exp. 6. Remember something and write a fuller description than you have previously written.

"Conscious of movement, not of its nature. Thought casually as I was reading, of recent visit to Carmel, and of a man who talked interminably, saying nothing." Evidently the execution of the task given was beyond the observer's subconscious mind.

Exp. 7. Remember something.

("snow snow snownowshoes Salmon Lake Happy Jack snow Denny") "I felt the long backward movement of hand, and once or twice movement of the fingers. I did not know what was written until I saw it. I was in Sierra Co., in December, in ten feet of snow, without snow shoes. Stayed at Salmon Lake Camp. Happy Jack is a stage-driver with whom I rode. 'Denny' was a man who worked for me several days. We improvised snow-shoes from pieces of board."

Exp. 8. Put pencil in his hand and sheet of paper before him. Told him to read in Emmy Lou and not to think of writing, or not writing.

("war with Mexico iniquitous Villa Wilson procras") "I felt a slight discomfort in the arm, and knew there were occasional movements. I was conscious of the word 'Mexico,' at least three times repeated; it was heard as though I spoke it inside the head, not through the outer ear. I do not know at all what I wrote." I did not show him what he had written even after his introspections had been written.

Exp. 9. Instructions of Exp. 8 repeated.

("Wilson silly Bryan is a chump") "I knew only that my hand was moving."

Exp. 10. Remember a man. Write complete sentences.

("Saw Henri Bardon at Indep riding bay") "I was conscious of movement of the hand, not of what was written. I knew the man Henri Bardon, near Independence, Inyo County, some years ago. He was a splendid horseman and rode a vicious bay colt. I have seldom thought of him since. I did not recall him until I saw what I had written."

Exp. 11. Asked him to write words of Exp. 10 as he would normally write them. Used the same pencil, same position, etc. It will be seen his automatic writing is not greatly different from his ordinary writing.

Exp. 12. Image a man.

("Salmon River Indian white hair verrrry dirty crooked nose") "As usual, I was conscious of movement, not of the writing. As I read, I had a visual image [8] of a map of the north-west part of the state, including Del Norte and parts of Siskiyou and Trinity Counties. I excluded this image as soon as formed. The map was colored similar to maps in the old school geographies. I do not know what I wrote." After seeng what he had written he said he spent last winter on Salmon River in Siskiyou County. Does not remember this Indian.

[8] Compare Prince: The Subconscious, p. 169.

Exp. 13. Remember a church.

("Small square seven logs high shake roof") "Saw several pictures of Stirling City, a lumber-camp and mill-town in Butte Co., but no church. I was conscious of movement and of cramped position of hand; not of what was written." After seeing what he had written he said: "They built a church at Stirling City after I left the town; I have never seen it, but have heard it described. This description corresponds with what I have heard of the church."

Exp. 15. Give the inversion temperature of nephilite to carnegisite. Has used this temperature in connection with his work. Says he knows it is between 1150 and 1400.

("1246°") "I was conscious of movement. Before I began to write I heard numbers—1320, 1328, 1370, etc., all above 1300." Book shows this temperature point lies between 1245° and 1252°. Aver. = 1248°.

Exp. 16. Give the boiling point of tin. Observer knows it is under 400°. He thinks 260°. Says he has often read the boiling point in papers from the Geophysical Laboratory at Washington.

("224°"). Boiling point of tin is 231 9/10.

Exp. 17. Told to hold hand still and remember something.

Exp. 18. Remember some forgotten experience of your childhood.

("Susan cried Ruth ate all her cake She get got some m") Before seeing the writing he wrote, "A glimpse of the path to my grandmother's house, with currant bushes at one side, in soft sunlight. No persons seen or heard."

With a view to combining the visual image method and the writing method I told the observer to assume a passive attitude. "The same path, with three rows of currant bushes; a tree, with general shape of an oak, with a swing. Grandmother's house in the distance. Carrie Looney walking on the path. Hollis Looney some distance behind." On seeing writing, said he could recall no such circumstances.

Exp. 19. Repeated 17.

("New ssshoes got John rowed boat.") "A bit of the road in front of grandmother's house, where it crossed the railroad; looking toward the river. A glimpse of a quarry near the river. No auditory images. A sense of slow movement in arm." On seeing the writing did not recall the experience but remarked on the next experiment day that "Some three hours after the experiment, I recalled that on one occasion, my father, the man John, some other people and I, were in a boat on the river. He was awkward and splashed the water. I was disturbed, fearing that my new shoes, of which I was proud, would be wet."

Exp. 20. Write some Latin sentence previously seen but forgotten.

("O olml write to Orrin Fairfield Is will dead") "I heard the scratch of the pencil, and was much more conscious of the movements than usual, and I was not surprised when I saw what I had written. The friend, "Will," I have not heard from for several years. Three days ago I dreamed an unusual dream about him and he has been much in my mind since. Particularly I have wondered if he were perhaps dead. I have thought of writing to his wife's people, the Fairfields."

Exp. 21. Remember something.

("Dead mouse Ruth found dead owl Burrried them") Before seeing the writing he wrote, "I was conscious of the movements, as usual. Toward the end I was more or less conscious of an association of the writing with a series of

strong memory images that arose to the exclusion of my reading. I do not know, however, what words I wrote. A corner of the pasture back of grandmother's house, next the hills; a little creek, tumbling down over some rocks; birch trees and bushes, and a bank of ferns. Here we had a grave yard, and Ruth, Jessie, Ruby and I buried all sorts of things with much ceremony,—a broken doll, a dead mouse, a cat, etc. Also a little boy from across the street played with us occasionally, whose name I do not recall." Did not remember the owl they buried until he saw the word written on the paper.

Exp. 22. Remember something.

("Johnie Glenn Johohnie Glenn") "I was not at all conscious of the writing, only vaguely of the movements, being interested in the book. I do not know what I wrote." On seeing the writing said, "Johnie Glenn was the name of the little boy across the street, whose name I was trying to remember in the last experiment but could not and whom I referred to in the previous experiment." [9]

Exp. 23. Write name of my mother.

Exp. 24. Write me the name of my uncle who died in California.

("No No") "Conscious of movement but did not know what I wrote or had I 'no' in mind."

Exp. 25. Give me some incident of my life in Indianapolis.

"I felt a slight twitching in the fingers, and then became aware of a vague feeling of discomfort, of being somewhat troubled and uncertain."

Exp. 26. Repeated Exp. 23. Evidently he has no "clairvoyant" power.

Exp. 27. Told to recall a room and to write automatically more fully than he had done usually.

("Two windows Red carpet wp flowers Black hair sofa Whatnot mustnt touch")

"A persistent, spontaneous image of a corner of the outside of my grandmother's house." Before showing him the writing I told him to allow a V-image to arise spontaneously of what he had written. "Three images arose: first, window of 'sitting room,' with canary in gilt cage; second, large heating stove in same room; third, corner of 'parlor' with 'What-not' of five shelves, with shells, etc."

It will be noticed that the writing increased in size as the experiments progressed in case of both M and O. This was due to the fact, doubtless, that not only the experimenter but the observer himself found it hard to decipher the early writing, partly on account of its small size. The experiments that follow are those made with O, who started to write at once without the preliminary hand twitchings that were observed in case of M. The automatic writing in connection with each experiment is given in Plate *B*.

[9] One is surprised from experiences like this that in experiments on memory so little attention has been given to the significance of the passive attitude as an aid to recall.

Exp. 1. After putting the pencil in observer's right hand she was told to read.

("Compress Did") "I was conscious of no movement except the ordinary movement felt in breathing. I do not know that I wrote or what I wrote."

Exp. 2. Image something.

("Wildness, alone never") "I was unaware of the formation of letters. I feel tired tonight but I did not consciously put it into words.

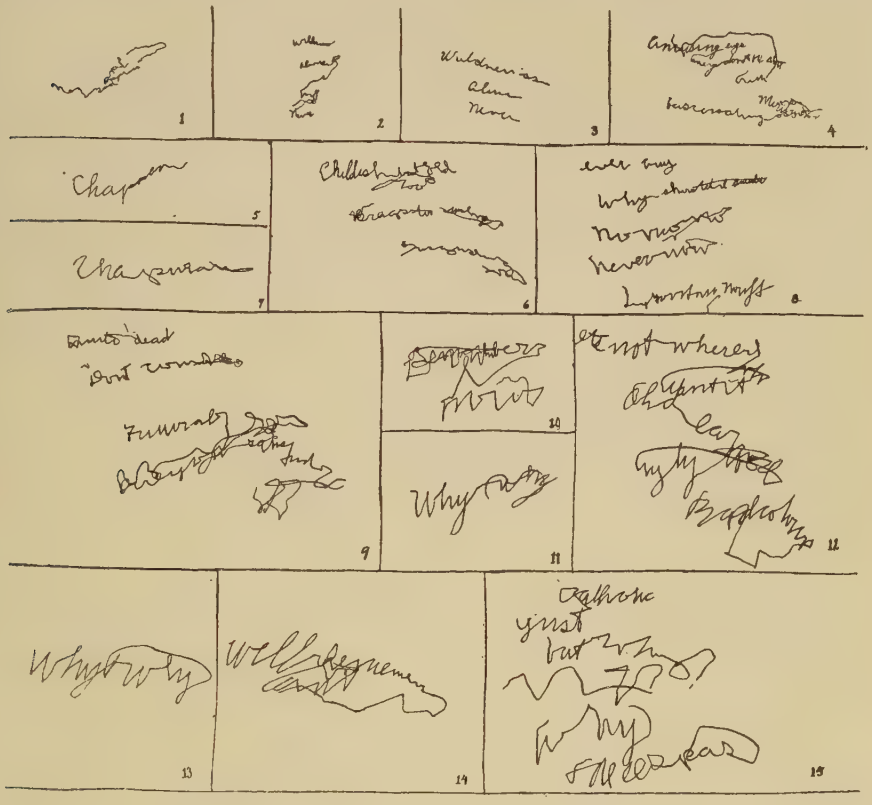


Plate *B*.— Observer O.

Exp. 3. Write the above words in your ordinary handwriting.

Exp. 4. Remember something.

("Amusing eyes. They do not tell the truth fascinating") "Merely conscious of movements due to breathing."

Exp. 5. Remember a man's name.

("Chapman") "I was unconscious of writing and had not thought of the particular name written."

Exp. 6. Remember a man.

("Childish but old too Brags too much Disgusting.") "Do not know the man referred to."

Exp. 7. Write the name of the man just described.

("Chapman") "There is a general feeling of annoyance but it was not in connection with the subjects written about, I do not think, but on account of the university work I have before me to do. Lately I have been troubled by dreams in which I was studying very hard. I have been awakened by my own talking."

As one looks over all the writing of O, one is impressed with the idea that what is uppermost under the threshold of consciousness during these experiments is not of a pleasant nature.

Exp. 8. Remember a pleasant experience of your childhood.

("Never any Why should I remember No No No Never now guten Nacht" [written in German script].) "Heard scratching movements of the pen but knew nothing farther of the writing. Have not had in these or the other experiments visual or auditory images. Have not written any German script for years."

Exp. 9. Image something.

("Quite dead Dont come Funeral Are you satisfied.") "I was reading an amusing incident in a kindergarten and was unconscious of any movement except the usual swaying movement which accompanies breathing."

Exp. 10. Write my mother's name.

("Remember now.")

Exp. 11. Write name of my uncle who died in California.

("why why.") "In neither of above cases did I expect to write anything. Was only conscious of breathing movements of hand."

Exp. 12. Remember church.

("Its not where I want it Oh dear Ugly Bad color.")

Exp. 13. Write the name of the church described in last experiment.

("why why")

Exp. 14. Repeated Exp. 12.

("well cant remember.")

Exp. 15. Repeated Exp. 12.

("Just Catholic but why, why neces pas.") "I did not expect to write automatically and was in every case surprised at the results. I heard the scratching of the pencil at times but was not aware that I was writing words."

The above experimental results show (1) that while theoretically the subconscious experience is reproduced through automatic writing without entering consciousness, to be certain that this actually occurred, that is, to be certain that the experience did not enter consciousness and after such entrance more or less influence and direct the writing, one must have observers who have the ability and the training to introspect very accurately. (2) That the image method has a much wider applicability, as it can be employed with any one who has visual and other images, while the automatic writing method, as is shown by these experiments and by others, is very limited in its application. In these experiments only two out of the 19 persons were really able to respond to the task set. (3) The image method gives more information in a given period of time and thereby decreases the difficulty of the introspection.

(4) In the image method the experience is brought above the threshold and the observer is encouraged to give his full attention to what occurs, and he may be directed to observe particular things. (5) In the image method it is not necessary to direct the movement connected with the giving of the information into an entirely new channel by substituting the action of lower nerve centers (centers connected with subconscious thinking) for the higher (centers connected with conscious thinking) which usually largely direct it. (6) In a confirmatory way the writing method may be made very useful. The great richness, for example, of what is under the threshold of consciousness in case of M and O is shown by both methods. (7) Each method also brings things to the attention not brought out by the other method. The tendency of the writing movements to be at the disposal of what is in consciousness is, for example, very noticeable in case of some observers. In case of M and O what is below the threshold evidently plays also a rôle as regards the writing.

Automatic Speaking Method Versus the Image Method.

Of some special cases of automatic speaking I have given illustrations in my study entitled "Die Projektionsmethode" (p. 5, 105). From what is heard by the patient himself or by the experimenter, an idea can be obtained of course of what is going on under the threshold of consciousness. The words occasionally unconsciously spoken by a normal person give one a similar idea. It will be at once evident, however, without any comparative experiments that the image method has a very much broader field of usefulness because of the difficulty of getting an adequate distraction in using the automatic speaking method.

The Image Method Versus the Pathological and the Psychoanalytical Methods of Investigating the Subconscious.

The other methods of investigating the subconscious I find less satisfactory than the automatic writing and speaking methods. The objection to the pathological method, where the data regarding the subconscious is obtained for example from cases of double personality, is the feeling of doubt and even mistrust with which one often collects and examines such data.

The objection to the method of psychoanalysis is that the instruction given to the patient to speak out everything that comes into his mind, gives a mass of data which contains not only what is below but

what is above the threshold and farther that in applying this method no systematic effort is made, as in the case of the image method, to separate out and classify such data.

Taken all in all, it seems to me, the results show that the image method offers a mode of penetrating below the threshold of consciousness which is at least comparable if not superior to that offered by other methods.

APPENDIX.

APPENDIX A.

TABLES.

TABLE XIII*a*.* (*Vide*, p. 55, *supra*).
Right Guesses on Playing-Cards.
Reduced to Sets of 50.

A. Normal Reagents.

		Card Not Imaged.					Card Imaged.				
		Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
Probable Number ...		1.25	25	5	12.5	22.5	1.25	25	5	12.5	22.5
	Reagent										
1st 1000	*1*.....	4	24	5	15	25	3	24	8	12	23
	2.....	3	28	5	15	21	0	19	6	6	28
	3.....	2	38	5	20	11	2	26	5	11	23
	4.....	2	23	5	14	24	2	26	7	12	19
	5.....	2	23	6	9	23	1	18	3	11	29
	6.....	2	30	6	12	16	0	27	4	10	21
	7.....	1	19	3	12	30	2	28	3	12	22
	8.....	0	25	5	13	22	0	20	2	13	28
	9.....	0	26	3	11	20	1	21	4	12	26
	10.....	2	28	4	14	21	1	26	5	16	20
Totals		18	264	47	135	213	12	235	47	115	239
2d 1000	*11*.....	1	25	8	14	20	2	26	8	12	18
	12.....	1	26	7	13	18	1	29	8	15	19
	13.....	2	20	7	8	27	1	18	4	7	30
	14.....	0	26	3	11	22	1	21	6	9	24
	15.....	1	28	5	12	20	0	25	4	14	23
	16.....	1	23	4	14	25	1	29	6	15	19
	17.....	3	25	6	11	23	1	25	4	19	22
	18.....	1	23	7	9	23	1	19	2	11	30
	19.....	2	25	10	17	19	2	24	6	11	22
	20.....	0	24	6	11	21	1	28	5	15	20
Totals		12	245	63	120	218	11	244	53	128	227

* Deviations from this table are used in Table XIV (pp. 57 ff.). The reductions were made by slide-rule from Table XIII (pp. 56 ff.) Only in this Table can the gross numbers of Right cases made by the respective reagents be compared.

TABLE XIIIa—Continued.

		Card Not Imaged.					Card Imaged.				
		Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
Probable Number ...		1.25	25	5	12.5	22.5	1.25	25	5	12.5	22.5
	Reagent										
3d 1000	*21*.....	1	20	2	7	30	2	20	6	12	26
	22.....	0	32	1	13	18	1	19	3	7	28
	23.....	0	26	1	17	23	2	21	5	11	25
	24.....	0	29	7	13	19	1	26	6	15	21
	25.....	4	30	9	19	17	4	29	11	16	18
	26.....	0	27	1	19	22	4	26	9	13	21
	27.....	0	18	1	11	32	2	27	7	15	18
	28.....	2	23	3	15	26	1	25	5	14	22
	29.....	0	24	2	15	25	0	28	4	7	20
	30.....	0	20	1	10	28	3	25	10	15	21
Totals		7	249	28	139	240	20	246	66	125	220
4th 1000	*31*.....	0	18	4	9	29	2	29	6	16	19
	32.....	5	25	8	17	25	6	27	10	20	20
	33.....	2	30	5	17	17	2	32	5	20	18
	34.....	2	26	4	16	22	1	31	6	14	15
	35.....	1	25	3	16	24	0	27	3	12	22
	36.....	1	30	8	15	16	2	29	5	17	21
	37.....	2	35	5	21	13	0	20	6	11	25
	38.....	0	20	4	9	27	2	26	4	10	23
	39.....	0	20	2	10	28	1	25	3	14	24
	40.....	0	24	1	11	26	4	27	6	16	22
Totals		13	253	44	141	227	20	273	54	150	209
5th 1000	*41*.....	1	30	4	11	17	3	25	9	10	21
	42.....	1	24	2	13	24	3	25	10	12	19
	43.....	2	32	4	12	18	0	24	4	12	23
	44.....	0	27	4	14	19	1	24	7	12	21
	45.....	1	24	6	10	23	0	29	3	15	19
	46.....	2	29	8	11	16	2	21	4	13	28
	47.....	0	28	1	14	22	2	28	8	15	17
	48.....	1	27	5	11	20	0	27	5	15	22
	49.....	1	27	8	12	19	4	25	11	17	22
	50.....	1	25	3	9	24	1	27	5	12	20
Totals		10	273	45	117	202	16	255	66	133	212

TABLE XIII*a*—Continued.

		Card Not Imaged.					Card Imaged.				
		Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
Probable Number ...		1.25	25	5	12.5	22.5	1.25	25	5	12.5	22.5
	Reagent										
6th 1000	*51*.....	4	20	7	14	28	2	22	6	13	25
	52.....	3	33	5	13	17	2	26	7	18	21
	53.....	1	25	6	11	22	1	26	5	12	20
	54.....	2	23	7	15	23	1	23	4	13	24
	55.....	2	23	4	10	24	1	19	2	10	30
	56.....	0	26	7	9	21	2	32	5	18	16
	57.....	2	20	5	7	27	3	21	6	14	25
	58.....	1	27	9	11	18	1	21	5	12	26
	59.....	1	36	5	21	13	4	26	8	11	20
	60.....	1	27	4	12	20	2	26	7	15	22
Totals		17	260	59	123	213	19	242	55	136	229
7th 1000	*61*.....	1	26	6	14	20	0	22	0	12	28
	62.....	1	30	2	13	20	0	29	4	9	19
	63.....	2	26	7	16	22	4	25	6	19	23
	64.....	6	31	9	17	18	3	26	7	14	24
	65.....	2	24	6	12	24	1	24	3	11	23
	66.....	3	25	8	10	25	3	24	11	15	20
	67.....	2	29	6	17	19	0	18	4	12	28
	68.....	2	25	8	10	21	2	27	5	15	22
	69.....	2	25	3	16	24	2	26	5	12	22
	70.....	2	25	8	12	22	2	18	5	11	29
Totals		23	266	63	137	215	17	239	50	130	238
8th 1000	*71*.....	2	25	2	13	25	1	21	5	14	28
	72.....	2	22	6	13	26	0	25	3	8	25
	73.....	1	29	5	19	19	1	20	6	9	26
	74.....	3	29	6	15	18	0	29	2	15	20
	75.....	0	28	8	13	16	2	23	6	14	24
	76.....	3	28	10	13	19	1	25	4	12	23
	77.....	1	29	5	13	18	2	17	7	9	28
	78.....	2	27	3	17	21	5	23	7	15	26
	79.....	0	16	5	9	31	2	26	3	13	23
	80.....	0	27	3	11	21	0	26	3	14	21
Totals		14	260	53	136	214	14	235	46	123	244

TABLE XIII*a*—Continued.

		Card Not Imaged.					Card Imaged.				
		Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
Probable Number ...		1.25	25	5	12.5	22.5	1.25	25	5	12.5	22.5
	Reagent										
9th 1000	*81*.....	1	20	3	9	29	1	26	4	12	21
	82.....	0	27	3	17	20	1	26	4	13	23
	83.....	2	26	5	14	23	1	23	6	12	23
	84.....	2	28	8	12	17	1	29	6	14	19
	85.....	1	18	4	9	30	1	26	5	11	20
	86.....	1	31	4	13	17	3	30	6	17	17
	87.....	0	24	4	8	22	1	23	6	10	24
	88.....	2	32	4	13	17	0	23	2	12	27
	89.....	0	31	4	15	17	2	23	3	10	26
	90.....	1	21	5	11	26	1	24	7	10	20
Totals		10	258	44	121	218	12	253	49	121	220
10th 1000	*91*.....	2	26	5	14	21	1	30	3	17	18
	92.....	1	22	5	11	27	1	20	8	12	24
	93.....	4	26	6	10	22	1	25	4	13	24
	94.....	1	27	5	15	22	0	29	1	13	21
	95.....	1	19	6	11	28	1	21	6	16	25
	96.....	1	22	5	6	28	0	29	2	16	20
	97.....	2	24	3	11	26	4	24	4	13	26
	98.....	1	20	5	14	28	2	23	5	9	26
	99.....	3	23	7	17	25	2	26	5	16	22
	100.....	3	22	5	9	25	0	32	4	21	16
Totals		19	231	52	118	252	12	259	42	146	222
B. Psychics.											
	1.....	0	23	8	8	23	2	27	5	13	20
	2.....	0	22	2	11	26	2	24	8	11	22
	3.....	3	30	6	15	18	4	26	6	14	24
	4.....	1	24	3	13	23	2	26	7	11	19
	5.....	0	26	2	17	22	2	29	4	18	20
	6.....	1	27	3	18	23	2	20	5	7	28
	7.....	3	26	7	16	20	0	31	4	16	17
	8.....	2	23	6	18	23	3	29	6	14	19
	9.....	0	22	3	8	25	0	23	5	13	23
	10.....	1	23	4	14	25	1	29	5	15	18
Totals		11	246	44	138	228	18	264	55	132	210

TABLE XIII*a*—Continued.

C. Corneal Reflection.

	Card Not Imaged					Card Imaged				
	Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
Probable Number	1.25	25	5	12.5	22.5	1.25	25	5	12.5	22.5
Reagent										
I.....	2	22	6	12	24	22	38	32	24	12
II.....	1	22	10	12	21	30	42	34	35	8
III.....	0	24	2	13	25	26	37	35	28	10
IV.....	4	21	10	13	26	24	47	31	33	3
V.....	1	31	6	14	15	9	32	19	16	15
Totals	8	120	34	64	111	111	196	151	136	48

TABLE XV*a*.*
Aggregates of R Cases.
Reduced to Sets of 50.

A. Students.

	Card Not Imaged.					Card Imaged.				
	Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
1st 1000	18	264	47	135	213	12	235	47	115	239
2d 1000	12	245	63	120	218	11	244	53	128	227
3d 1000	7	249	28	139	240	20	246	66	125	220
4th 1000	13	253	44	141	227	20	273	54	150	209
5th 1000	10	273	45	117	202	16	255	66	133	212
6th 1000	17	260	59	123	213	19	242	55	136	229
7th 1000	23	266	63	137	215	17	239	50	130	238
8th 1000	14	260	53	136	214	14	235	46	123	244
9th 1000	10	258	44	121	218	12	253	49	121	220
10th 1000	19	231	52	118	252	12	259	42	146	222
Totals 143		2559	498	1287	2212	153	2481	528	1307	2260
B. Psychics ...	11	246	44	138	228	18	264	55	132	210
C. Corneal Reflection..	8	120	34	64	111	111	196	151	136	48

* Comparable to Table XV (p. 64), but based on Table XIII*a*.

TABLE XVI*a**.
Per Cents of R Cases.

A. Students

	Card Not Imaged					Card Imaged				
	Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
1st 1000	3.60	52.8	9.4	27.0	42.6	2.40	47.0	9.4	23.0	47.8
2d 1000	2.40	49.0	12.6	24.0	43.6	2.20	48.8	10.6	25.6	45.4
3d 1000	1.40	49.8	5.6	27.8	48.0	4.00	49.2	13.2	25.0	44.0
4th 1000	2.60	50.6	8.8	28.2	45.4	4.00	54.6	10.8	30.0	41.8
5th 1000	2.00	54.6	9.0	23.4	40.4	3.20	51.0	13.2	26.6	42.4
6th 1000	3.40	52.0	11.8	24.6	42.6	3.80	48.4	11.0	27.2	45.8
7th 1000	4.60	53.2	12.6	27.4	43.0	3.40	47.8	10.0	26.0	47.6
8th 1000	2.80	52.0	10.6	27.2	42.8	2.80	47.0	9.2	24.6	48.8
9th 1000	2.00	51.6	8.8	24.2	43.6	2.40	50.6	9.8	24.2	44.0
10th 1000	3.80	46.2	10.4	23.6	50.4	2.40	51.8	8.4	29.2	44.4
Totals	2.86	51.2	10.0	25.7	44.2	3.06	49.6	10.6	26.1	45.2
B. Psychics	2.20	49.2	8.8	27.6	45.6	3.60	52.8	11.0	26.4	42.0
C. Corneal Reflection	3.20	48.0	13.6	25.6	44.4	44.40	78.4	60.4	54.4	19.2
Probable	2.50	50.0	10.0	25.0	45.0	2.50	50.0	10.0	25.0	45.0

TABLE XVII*a***.
Deviations from Probable Per Cent.
Reduced to Sets of 50.

A. Students

	Card Not Imaged					Card Imaged				
	Card	Color	No.	Suit	W	Card	Color	No.	Suit	W
1st 1000	+0.90	+2.8	−0.6	+2.0	−2.4	−0.10	−3.0	−0.6	−2.0	+2.8
2d 1000	−0.10	−1.0	+2.6	−1.0	−1.4	−0.30	−1.2	+0.6	+0.6	+0.4
3d 1000	−1.10	−0.2	−4.4	+2.8	+3.0	+1.50	−0.8	+3.2	0	−1.0
4th 1000	+0.10	+0.6	−1.2	+3.2	+0.4	+1.50	+4.6	+0.8	+5.0	−3.2
5th 1000	−0.50	+4.6	−1.0	−1.6	−4.6	+0.70	+1.0	+3.2	+1.6	−2.6
6th 1000	+0.90	+2.0	+1.8	−0.4	−2.4	+1.30	−1.6	+1.0	+2.2	+0.8
7th 1000	+2.10	+3.2	+2.6	+2.4	−2.0	+0.90	−2.2	0	+1.0	+2.6
8th 1000	+0.30	+2.0	+0.6	+2.2	−2.2	+0.30	−3.0	−0.8	−0.4	+3.8
9th 1000	−0.50	+1.6	−1.2	−0.8	−1.4	−0.10	+0.6	−0.2	−0.8	−1.0
10th 1000	+1.30	−3.8	+0.4	−1.4	+5.4	−0.10	+1.8	−1.6	+4.2	−0.6
Total	+0.36	+1.2	0	+0.7	−0.8	+0.56	−0.4	+0.6	+1.1	+0.2
B. Psychics	−0.30	−0.8	−1.2	+2.6	+0.6	+1.10	+2.8	+1.0	+1.4	−3.0
C. Corneal Reflection	+0.70	−2.0	+3.6	+0.6	−0.6	+41.90	+28.4	+50.4	+29.4	−25.8

* Comparable to Table XVI (p. 64), but based on Table XV*a*.

** Comparable to Table XVII (p. 65), but based on Table XVI*a*. This and the preceding Table show by their close approximations to their corresponding Tables XVI and XVII that the "reduction to sets of 50" has not introduced any important errors. Consequently the values in Table XIV (pp. 57.ff.) may be confidently inspected.

TABLE XLIX*a*.*

Statistical Expectation, expressed in Per Cent of R Cases, after the experiments were concluded. (52 Reagents).

Reagent	Card	Color	No.	Suit	Reagent	Card	Color	No.	Suit
32	6	60	17	60	*64*	5	50	10	25
33	5	75	12	40	*65*	5	70	15	40
34	6	65	40	65	*67*	5	75	13	30
35	6	70	15	30	*68*	5	65	15	38
36	10	75	15	50	*70*	8	70	15	30
37	10	70	30	45	*71*	3	60	15	25
38	5	60	15	33	*72*	4	60	15	30
39	5	60	12	25	*73*	3	50	10	50
41	5	60	10	25	*75*	10	70	15	40
42	15	75	35	65	*76*	10	70	20	35
43	7	55	12.5	40	*79*	5	80	10	40
44	3	60	12	50	*81*	3	50	25	45
45	10	80	25	50	*82*	10	50	10	30
47	7	60	15	35	*83*	3	65	10	35
48	6	60	12	30	*84*	8	75	30	60
49	5	70	14	35	*86*	11	70	25	35
50	3	52	10	30	*87*	5	75	12	50
53	3	60	12	30	*88*	8	70	25	40
54	6	60	10	28	*89*	5	60	15	25
55	3	55	10	25	*90*	10	70	10	25
56	5	60	15	35	*91*	10	75	10	25
57	4	50	20	25	*92*	5	60	14	30
58	5	60	25	35	*93*	5	60	30	35
61	10	65	23	35	*94*	3	75	20	30
62	10	70	20	50	*95*	2.5	70	15	50
63	5	50	15	30	*98*	2.5	60	25	40

	Card	Color	No.	Suit	W
Totals	319	3342	880.5	1939	1457.75
Average	6.14	64.3	16.9	37.2	28.0
% Dev. (100).....	+3.64	+14.3	+6.9	+12.2	−17.0
% Dev. (50).....	7.28	28.6	13.8	24.4	−34.0

*In Plate X (p. 111), the first curve was drawn from the last line of the table.

TABLE XLIX*b*.*

Expectation of 52 Reagents distributed with Probability.

	Card					Number			
x	E	E_1	P	P_1	x	E	E_1	P	P_1
0	0		7.96		0	0		0	
1	0	0	20.4	28.4	1	0		0.02	
2	3.8		25.9		2	0	0	0.16	0.2
3	17.3	21.1	21.7	47.6	3	0		0.6	
4	3.8		13.5		4	0		1.6	
5	32.7	36.5	6.6	20.1	5	0		3.4	
6	9.6		2.7		6	0		6.0	
7	3.8	13.4	0.9	3.6	7	0	0	8.9	20.5
8	5.8		0.28		8	0		11.5	
9	0	5.8	0.07	0.3	9	0		13.0	
10	19.2		0		10	21.2		13.2	
11	1.9	21.1	0	0	11	0		12.0	
12	0		0		12	13.5	34.7	9.8	59.5
13	0	0	0	0	13	1.9		7.4	
14	0		0		14	3.8		5.1	
15	1.9	1.9	0	0	15	26.9		3.3	
					16	0		1.9	
	99.8	99.8	100.01	100.0	17	1.9	34.5	1.1	18.8
					18	0		0.5	
					19	0		0.3	
					20	7.7	7.7	0.1	0.9
					23	1.9		0	
					25	11.6	13.5	0	0
					30	5.8	5.8	0	0
					35	1.9	1.9	0	0
					40	1.9	1.9	0	0
						100.0	100.0	99.88	99.9

*The distributions in this table were used to draw the curves in Plate XI (p. 111).

TABLE XLIX*b*—Continued.

Expectation of 52 Reagents distributed with Probability.

	Color					Suit			
x	E	E_1	P	P_1	x	E	E_1	P	P_1
33	0		0.023		8	0		0.001	
34	0		0.046		9	0		0.003	
35	0		0.087		10	0		0.009	
36	0		0.156		11	0		0.026	
37	0	0	0.270	0.58	12	0	0	0.063	0.10
38	0		0.448		13	0		0.142	
39	0		0.711		14	0		0.294	
40	0		1.09		15	0		0.563	
41	0		1.59		16	0		0.998	
42	0	0	2.23	6.07	17	0	0	1.650	3.65
43	0		3.01		18	0		2.53	
44	0		3.90		19	0		3.64	
45	0		4.85		20	0		4.92	
46	0		5.80		21	0		6.25	
47	0	0	6.66	24.22	22	0	0	7.49	24.83
48	0		7.35		23	0		8.46	
49	0		7.81		24	0		9.06	
50	11.6		7.96		25	17.3		9.18	
51	0		7.81		26	0		8.83	
52	1.9	13.5	7.35	38.28	27	0	17.3	8.07	43.60
53	0		6.66		28	1.9		7.01	
54	0		5.80		29	0		5.80	
55	3.8		4.85		30	21.2		4.58	
56	0		3.90		31	0		3.45	
57	0	3.8	3.01	24.22	32	0	23.1	2.48	23.32
58	0		2.23		33	1.9		1.70	
59	0		1.59		34	0		1.12	
60	32.7		1.09		35	17.3		0.704	
61	0		0.711		36	0		0.424	
62	0	32.7	0.448	6.07	37	0	19.2	0.244	4.19
63	0		0.270		38	1.9		0.135	
64	0		0.156		39	0		0.072	
65	7.7		0.087		40	13.5		0.036	
66	0		0.046		41	0		0.018	
67	0	7.7	0.023	0.58	42	0	15.4	0.008	0.27
70	23.1	23.1	0	0	45	3.8	3.8	0	0
75	15.4	15.4	0	0	50	13.5	13.5	0	0
80	3.8	3.8	0	0	55	0	0	0	0
					60	3.8	3.8	0	0
	100.0	100.0	100.022	100.02	65	3.8	3.8	0	0
						99.9	99.9	99.960	99.96

LISTS OF NONSENSE-SYLLABLES.

(See text, p. 374, footnote 8.)

The "pav" List.

	1	2	3	4	5
1	pav	bote	brum	krig	friss
2	tez	dotch	plike	struzz	skell
3	cheen	jeek	sprote	dhane*	hib
4	kire	gafe	thell	klore	whaj
5	fabe	vem	blup	skradd	pixe
6	soje	zile	troitch	fless	tept
7	lig	roos	slafe	grupp	chakt
8	med	nalt	shabe	sklatt	kimpt
9	wung	pronch	splej	spritch	fint
10	yajd	vrap	drav	glick	seft

	6	7	8	9	10
1	ligd	nevd	moosh	grun	flates
2	mitcht	yast	wimd	klare	dhald
3	wazd	kosp	vumpt	frast	sprix
4	bort	chith	zogd	gluf	shond
5	dant	pelj	reng	thisk	whart
6	joord	bulf	brud	skeft	spand
7	gand	tusp	plaz	splush	trutcht
8	vobde	fask	krim	hoovd	drazd
9	zulb	sadhe	prag	stalt	dempt
10	rald	leets	bliv	nept	junt

The "shug" List.

	1	2	3	4	5
1	shug	pudh*	sug	lun	vus
2	duf	duzh	lus	tuz	judh
3	gub	bup	nuth	rud	kush
4	chud	zuk	thush	guk	nug
5	thul	dhuv	shuf	chut	zul
6	tus	fush	kutch	buth	guj
7	vutch	vum	puzh	zhul	muv
8	kun	chuz	fub	ruv	tur
9	sut	mudh	zhum	tuj	zhuf
10	ruj	jur	dup	dhur	nup

*Vocalized *th*, as in *the*, is written *dh*.

THE "SHUG" LIST—Continued.

	6	7	8	9	10
1	süth	fup	dush	dhug	dhub
2	lut	kuzh	thuv	zhud	shul
3	shub	pum	jun	tul	fum
4	juk	suf	rutch	fudh	zhup
5	zum	zug	zud	puj	thun
6	buzh	mub	budh	dut	gutch
7	dhun	chus	puz	juf	vur
8	kuz	nuk	guth	rus	nuzh
9	mur	thut	lutch	buv	chuth
10	shud	luj	vuk	sush	muz

THE "SU" AND "UR" LISTS.

	1	2	3	4	5	6	7	8	9	10
1 ..	su	gu	du	zu	mu	chu	shu	zhu	bu	ju
2 ..	ku	vu	fu	tu	thu	dhu	nu	pu	lu	ru
3 ..	ru	lu	pu	nu	dhu	thu	tu	fu	vu	ku
4 ..	ju	bu	zhu	shu	chu	mu	zu	du	gu	su
5 ..	su	ku	ru	ju	su	gu	vu	lu	bu	ku
6 ..	du	fu	pu	zhu	gu	zu	tu	nu	shu	vu
7 ..	mu	thu	dhu	chu	du	chu	dhu	thu	mu	fu
8 ..	shu	nu	tu	zu	zu	zhu	pu	fu	du	tu
9 ..	bu	lu	vu	gu	mu	ju	ru	ku	su	thu
10 ..	chu	shu	zhu	bu	ju	dhu	nu	pu	lu	ru

	1	2	3	4	5	6	7	8	9	10
1 ..	ur	ul	up	un	udh	uj	ub	uzh	ush	uch
2 ..	uth	us	uk	ur	uj	um	ug	uv	ul	ub
3 .	ut	ud	uf	up	uzh	uz	uz	ut	un	ush
4 ..	uf	um	uth	udh	uch	ud	uch	udh	uth	um
5 ..	uv	ush	un	ut	uz	ug	uzh	up	uf	ud
6 ..	uk	ub	ul	uv	ug	us	uj	ur	uk	us
7 ..	us	ug	ud	uz	um	uch	ush	uzh	ub	uj
8 ..	uk	uv	uf	ut	uth	udh	un	up	ul	ur
9 ..	ur	ul	up	un	udh	uth	ut	uf	uv	uk
10 ..	uj	ub	uzh	ush	uch	um	uz	ud	ug	us

Pronounce ch as in churn or such,
" sh " " shut " brush,
" zh " " azure
" s " " so " us,

Pronounce j as in jump or judge,
" g " " gulf " cog,
" th " " thud " myth,
" dh " " thus " scythe.

APPENDIX B.

EXPERIMENTS IN LONG-DISTANCE THOUGHT-TRANSFERENCE.

It is much to be desired that investigators should give attention to obtaining more results in this branch of the inquiry. For independently of the fact that results of the kind form an indispensable link between instances ot thought-transference at close quarters and the more striking spontaneous cases at a distance, it is important to observe that in experiments of the kind described in the present chapter the gravest objection which is at present urged, and may fairly continue to be urged, against most experiments at close quarters—viz., the risk of unconscious apprehension through normal channels—is no longer applicable. Moreover, the results can only be attributed to fraud on the extreme assumption that both parties to the experiment are implicated in deliberate and systematic collusion.—FRANK PODMORE: Apparitions and Thought-Transference, 1894, pp. 106-7.

Further experiment is much to be desired, and those of your readers who are interested in the subject can confer no greater benefit on psychical research than by themselves carrying out experiments on thought transference at a distance, and so adding to the reliable criteria for telepathic communication.—MARGARET DE G. VERRALL: London *Daily News,* September 6, 1911; also *Journal S. P. R.,* October 1911, 15: 132.

EXPERIMENTS IN LONG-DISTANCE THOUGHT-TRANSFERENCE.

There have been reports in the professional literature of long-distance thought-transference, and ever since the time of Paracelsus [1] belief has been expressed that distance is no barrier to the transference of thought.

Consequently, when an opportunity occurred (February, 1914) some experimental results were gathered. Unfortunately, some of the prospective reagents did not find it possible to take the series, and others were unable to finish it. What data there are, however, are not without value, and the method of experiment merits record. Results from this method become comparable to the large amount of statistical data already accumulated and can therefore be readily evaluated and interpreted.

[1] "By the magic power of the will a person on this side of the ocean may make a person on the other side hear what is said on this side;" and "The ethereal body of a man may know what another man thinks at a distance of 100 miles and more."—Paracelsus.

Method.

The following letter was sent out to "sensitives" who had signified their interest and desire to aid in the investigation:

Leland Stanford Junior University

Department of Psychology,
Division of Psychical Research.

Stanford University, Calif., January 1914.

A personal note to the sensitive:

The accompanying instructions for an experiment in Long-Distance Thought-Transference will seem upon the first reading rather complicated. A second reading, together with a little study and a few trials by yourself, will, I trust, smooth things out.

You will understand that at 8 o'clock p.m., to the second, and at each 5-minute interval after, the period of impression is to begin; it should not last over one minute, and it may be shorter if you get a reliable impression in less time. Then you record a card (like R5H for Red five of hearts) in its proper place in the Record Sheet; then you write out your introspections on this single experiment. Be careful not to let the writing of introspections break into the period of impression, because that period corresponds to the time (one minute) during which the experimenter here (in Palo Alto) is holding the card pertinent to that experiment.

Common playing-cards are used; the face or court cards are discarded, leaving the 40 cards with spots 1 to 10.

After the card is drawn by chance for the experiment, the experimenter may look at it or not, as controlled by the odd numbers of a die which he shakes in a dice-box. Thus some opportunity will be given for clairvoyance. The experimenter keeps a record of the card, whether it was entertained in his mind, in what kind of imagery it was entertained (visual if like a picture seen, auditory if like a name heard, kinæsthetic if like an impulse of the vocal organs to speak the name of the card). In some definite cases the imagery of the card will be mixed.

Let the experimentation begin February 2d, and continue daily for all school days (thus omitting Saturday and Sunday) until and including February 13th, when, at the rate of ten each evening (a complete series) the whole set of 100 experiments will be complete. Since others are also taking the experiments, in San Francisco, in Healdsburg, in Richmond, etc., kindly permit nothing except the greatest necessity to break the appintments.

Before experimentation begins each evening, record in the last column on the Time Chart the number of seconds your watch is fast or slow: that will be the point at which, for the evening, your period of impression will begin; "time" must be got each day, and the watch set to the nearest minute. (In setting the watch keep the minute-hand with the second-hand, so that when the second-hand points to 60 the minute-hand will be precisely over the minute mark; then you will not be in danger of taking the wrong minute for the impression.) Keep a record of

all changes in regulating your watch, etc., as you are instructed on the sample Time Record.

Records may be made throughout in pencil. At each sitting the state of the health and mind should be recorded opposite the date, on the Record Sheet.

Your interest in scientific investigation is attested by your willingness to take part in this experimentation, and you may be assured that your results, with those of others, will help to determine something in regard to unrecognized mental (?) powers.

Very truly yours,
JOHN E. COOVER,
Fellow in Psychical Research.

Accompanying the letter were sent five inclosures:

1. Instructions for an experiment in Long-Distance Thought-Transference.
2. Sample Time Record.
3. Time Record blank.
4. Record Sheet.
5. Sheets for introspections.

1. Instructions for an experiment in Long-Distance Thought-Transference.

Precisely at 8 o'clock sit quietly and comfortably at a table or with a writing pad, clear the mind of all impressions and trains of thought, and settle down to a quiescent state alert only for the impression of some playing-card (with spots from 1 to 10). Use from a quarter to one minue in getting this impression.

Record the impression in the first space in the first column of the Record Sheet (it is the first experiment in the first series of ten) by color, number, and suit, (as, for the ten of hearts, R10H). Keep this record out of sight, henceforth, so as not to be influenced unconsciously by it; bring it into view only when you have to record another impression or guess. To do this you can keep it covered with the sheets for introspections.

Every experiment will be represented by a line on the Sheets for Introspections. The introspection is a description of your impression, and what you feel it to be worth; *e. g.*, the columns are headed 1, 2, a, b, c, d, 3, a, b, which are to be filled out on the appropriate line as follows:

1. Was the mind in a receptive mood during the interval of impression?
2. a. What kind of an impression was it: Visual (like a picture seen), auditory (as hearing a voice speak the name of the card), or kinaesthetic (impulse for the vocal organs to go through the motions of pronouncing the name of the card)?
   b. How vivid was the impression? (Grade A for high, D for low, B and C for intermediate grades).
   c. Did the impression come
      (1) At the beginning, middle, or end of the period?
      (2) Slowly or quickly?
      (3) Was it persistent or intermittent?

d. Record by initial where the picture or voice constituting the impression (if it is not merely an impulse to speak the name of the card) seems to be located (*i. e.*, if in front about reading distance, record: f. 1 ft.) using the terms, front, back, right, left. (This you note applies to visual and auditory impressions. The impulse to speak the name of the card is, of course, an impression located in the throat and mouth).

3. a. What do you feel that this impression is worth? How much better chance than a pure guess do you feel it has of being right? (Grade A for high, D for low, etc.)

b. If the grade is A or B, note briefly just why it seems so much better than others.

The introspections are to be written each time just after the card is recorded —use initials (as, b. for beginning). Practice by yourself for a few times before the experiments begin will make the matter easy for you. Anything that occurs to you as being important for which there is no provision on the sheets, kindly note on other paper, keeping the number of the experiment and of the series.

At five minutes after 8 o'clock the interval begins for another experiment; and thereafter every five minutes, until 10 are completed (a full series). The first evening only the first series will be done; on the second evening the second series of ten will be performed, etc. (A series on the Record Sheet is numbered at the top; the single experiments at the side. On the sheets for introspections both series and experiments are numbered at the side.) (Always be sure in writing introspections that your experiment number corresponds with that in your Record Sheet.)

After the impression period of not over a minute a card must be recorded, even if no impression was received; in this case no entries need be made in columns *2, a, b, c, d,* of the Sheet for Introspections, but in *3, a, b,* record "pure guess."

Put your name on each sheet of paper you use as a record.

Return all records and instructions after the experimentation is completed.

2. Sample Time Record.

February, 1914.

Date	Hour Time Compared*	Time of Watch	Watch set to	Period of Impression Begins
2	12	12- 3-20	12-00-20	20 sec. after the min.
3	12	12- 1-45	11-59-45	15 sec. before the min.
4	12	11-58-30	12-00-30	30 sec. after the min.

*The time when the "time" is taken may be 12 or 5 or any regular hour when telegraphic time is received at the telegraphic instrument (at Western Union Office) or is sounded by a municipal gong. (Here in Palo Alto the time is sounded by the first of three taps of a gong—heard all over town—at the hours of 8, 12, and 5).

In case one must resort to a clock controlled by telegraphic time, he should ascertain when the clock is automatically set, how much in error it is possible for it to be, satisfy himself by inquiry that he can get true time from it, and take time regularly at his most convenient hour, every day of experimentation.

It is obvious that the Time Record will establish the fact of coincidence for the subject's and experimenter's experimental periods, and will indicate what the extent of error may be. The record of the running of the watch is important. If the watch is regulated faster or slower, that fact should be recorded on the lower part of the Time Record Sheet, together with the date, hour, and direction of regulation (as provided on the Sheet).

It would be advisable to take time regularly several days before the experiments begin, and regulate the watch to reasonable accuracy; then refrain from regulation during the weeks of experimentation.

3. Time Record Blank.

February, 1914.

Date	Hour Time Compared	Time of watch	Watch set to	Period of impression begins (according to your own time)
2	....	.....	.....	.....
3	.....	.....	.....	.....
4	.....	.....	.....	.....
5	.....	.....	.....	.....
6	.....	.....	.....	.....
.....	.....	.....	.....	.....
9	.....	.....	.....	.....
10	.....	.....	.....	.....
11	.....	.....	.....	.....
12	.....	.....	.....	.....
13	.....	.....	.....	.....

WATCH WAS REGULATED.

Date	Hour	Faster or slower
.....	.....	.....
....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....
.....	.....	.....

4. Record Sheet.

Use initials, and record (1) color, (2) number, (3) suit; as, B3S for "Black Three of Spades."

Name.....

1. Date.....Condition..... 6. Date.....Condition.....
2. Date.....Condition..... 7. Date.....Condition.....
3. Date.....Condition..... 8. Date.....Condition.....
4. Date.....Condition..... 9. Date.....Condition.....
5. Date.....Condition.....10. Date.....Condition.....

	1	2	3	4	5
1	.....	.....	.....	.....	.....
2	.....	.....	.....	.....	.....
3	.....	.....	.....	.....	.....
4	.....	.....	.....	.....	.....
5	.....	.....	.....	.....	.....
6	.....	.....	.....	.....	.....
7	.....	.....	.....	.....	.....
8	.....	.....	.....	.....	.....
9	.....	.....	.....	.....	.....
10	.....	.....	.....	.....	.....

	6	7	8	9	10
1	.....	.....	.....	.....	.....
2	.....	.....	.....	.....	.....
3	.....	.....	.....	.....	.....
4	.....	.....	.....	.....	.....
5	.....	.....	.....	.....	.....
6	.....	.....	.....	.....	.....
8	.....	.....	.....	.....	.....
9	.....	.....	.....	.....	.....
10	.....	.....	.....	.....	.....

5. Sheets for Introspections.

Series	Experiments	1	2				3	
			a	*b*	*c*	*d*	*a*	*b*
1	1	.....	.....	.....	.....	.....	.....	.....
	2	.....	.....	.....	.....	.....	.....	.....
	3	.....	.....	.....	.....	.....	.....	.....
	4	.....	.....	.....	.....	.....	.....	.....
	5	.....	.....	.....	.....	.....	.....	.....
	6	.....	.....	.....	.....	.....	.....	.....
	7	.....	.....	.....	.....	.....	.....	.....
	8	.....	.....	.....	.....	.....	.....	.....
	9	.....	.....	.....	.....	.....	.....	.....
	10	.....	.....	.....	.....	.....	.....	.....
2	1	.....	.....	.....	.....	.....	.....	.....
	2	.....	.....	.....	.....	.....	.....	.....
	3	.....	.....	.....	.....	.....	.....	.....

Results.

Three reagents contributed results. The first, who lived in Healdsburg, about 90 miles from the experimenter's position, faithfully carried out the complete program, sitting an hour on each of the appointed evenings, and sent in the results of 100 experiments, with introspections. The second, who lived in the city of San Francisco, 27 miles away, sent in results of 36 experiments, and the third, who lived in Richmond, 35 miles away, sent in results of 13 experiments.

The results of the one complete set are given below:

	Card Not Imaged						Card Imaged					
	Card	Color	No.	Suit	W	Total	Card	Color	No.	Suit	W	Total
R Cases ...	5	23	10	13	19	46	0	32	2	15	21	54
Reduced* ..	5	25	11	14	20	50	0	30	2	14	19	50
Dev.	+3.75	0	+6	+1	−2		−1.25	+5	−3	+1	−3	

The deviations in the last line are comparable with those in Table XIV.[2]

*Both sides of the table reduced to even sets of 50 experiments.

[2] *Supra*, pp. 57 ff.

The results in aggregate for the three reagents follow:

	Card Not Imaged						Card Imaged					
	Card	Color	No.	Suit	W	Total	Card	Color	No.	Suit	W	Total
R Cases ...	5	36	12	19	26	67	0	47	5	23	33	82
%	7.46	53.7	17.8	28.4	38.8		0	57.4	6.1	28.0	40.2	
Dev.	+4.96	+3.7	+7.8	+3.4	−6.2		−2.5	+7.4	−3.9	+3.0	−4.8	

Both sides of table combined:

R Cases	5	83	17	42	59	149
%	3.35	55.7	11.4	28.2	39.6	
Dev.	+0.85	+5.7	+1.4	+3.2	−5.4	
Limit of Chance Dev.	+5.00	+17.	+10.	+15.	−17.	

It will be noted that all complete hits (5) occurred in the "Card Not Imaged" experiments and that on the same side of the table Number has the larger deviation (in the complete set) and Suit also (in the aggregate results). Therefore, the positive deviations point more strongly toward lucidity than toward thought-transference, in case they are to be considered of any weight. This is doubtful, however, since the deviations of all tables are exceeded by both inductive and theoretical chance deviations. When the two sides of the table are aggregated, as in the last table, in order to decrease the chance deviations by increasing the number of total cases, we find that the positive deviations decrease and that they do not exceed a third of the limit of chance deviations; about 16% of the positive chance deviations of a large number of sets would be found to be larger.

The Introspections show that judgments were based upon imagery in three modes, visual, auditory, and kinaesthetic, and that it was usually in indifferent intensity. There were a few highly graded judgments, however, in the completed set: Series 6, Experiment 10, in which the impression came vividly at the end of the interval in both auditory and visual forms; 9:1 and 9:10, in which it came in dual mode, also, quickly and vividly. These factors of certainty are among those pointed out by both the normal reagents and the psychics, in the larger series of experiments. These three judgments were right in two colors and one suit only. Of the five complete hits, three were given the lowest grade, and two were pure guesses. The psychic suggested several reasons why her series might not be satisfactory; (1) Time was not exact; (2) She was unfamiliar with playing-cards; (3) The time of impression (one minute) seemed to her rather short; (4) Hitherto messages had come to her simply as "mental impressions," not as visual or auditory imagery.

Yet, if the time was sufficiently accurate to insure coincidence of a substantial proportion of the interval of impression, as it seems to have been, the set should have offered a fair opportunity for thought-transference to show itself. One of the reagent's many experiences suggests that the distance, at least, need not have been prohibitive: in the year 1903 her husband left for Denver, about 960 miles distant, to be gone several weeks, leaving her alone in the house. One night, after she had retired very early and had fallen asleep, she was aroused by hearing, as she thought, her husband's voice calling her name from the adjoining room. She replied, asking what he wished, and received plainly the reply "I am all right." Then she realized that she was alone, heard the clock strike nine, and quickly fell to sleep again. The next day the papers reported the collapse of a great tent, in which she knew he would be, and the miraculous escape of many persons from death. She then understood the message. Upon his return he inquired if she got his message sent on the given night at ten o'clock, which, with allowance for difference in longitude, was considered coincident with her experience.

This method of experiment for long-distance thought-transference offers opportunity for proof of the objective existence of the phenomenon, and, also, because of the elimination of spatial difficulties, for proof of thought-transference between persons who consider each other especially *en rapport.*

APPENIDIX C.

GROUNDS FOR SCIENTIFIC CAUTION IN THE ACCEPTANCE OF THE "PROOF" OF THOUGHT-TRANSFERENCE.

For the benefit of the reader who has felt more or less dissatisfaction with the writer's reserve in his treatment of the telepathic hypothesis, or has read the quotations in the introduction to Part I [1] with impatience, it has been thought advisable to present here in the Appendix some of the more specific grounds for scientific caution; he will then be in a position himself to exercise his own judgment as to whether scientific caution in the acceptance of thought-transference as a fact is justified by a bit of the history of its investigation.

From the time of the "First Report on Thought-Reading" [2] by the Committee of the Society for Psychical Research, prominent psychical researchers, including the members of the Committee, have from time to time counseled caution in the acceptance of the current evidence as proof of thought-transference.

In that "First Report" communicated by Barrett, Gurney, and Myers, the Committee said:

> Hesitation in accepting any facts so novel, and, in many ways, suspicious, as mind-reading, is, of course, perfectly justifiable; and we are prepared to expect much criticism and prolonged experiment, before any generalization from the facts can meet with wide acceptance.[3]

In 1886 Gurney wrote:

> The part of the map that science leaves blank, as *terra incognita,* is the very one which amateur geographers will fill in according to their fancy, or on the reports of uncritical and untrustworthy explorers. The confidence of ignorance is always pretty accurately adjusted to the confidence of knowledge. Whenever the expert can put his foot down, and assert or deny with assurance, the uninstructed instinctively bow to him. He fearlessly asserts, for instance, that the law of the conservation of energy cannot be broken; the world believes him, and the inventors of perpetual-motion machines gradually die off. But suppose the question is of pos-

[1] *Supra,* pp. 3 ff.
[2] July 17, 1882. *Proceedings S. P. R.,* 1882, 1: 13 ff.
[3] *Op. cit.,* p. 17.

sible relations of human beings to inanimate things or to one another, new modes of influence, new forms of sensitiveness. Here responsible science can give no confident denial; here, therefore, irresponsible speculation finds its chance. It has, no doubt, modified its language under the influence of half a century of brilliant physical discovery. . . . Speculation here is not only easy; it is, unfortunately, also attractive. The more obscure phenomena and the more doubtful assumptions are just those on which the popular mind most readily fastens; and the popular tongue rejoices in terms of the biggest and vaguest connotation. Something also must be set down to a natural reaction. Even persons whose interest has been earnest and intelligent have found scientific *morale* hard to preserve, in departments surrendered by a long-standing convention to unscientific treatment. Thus, in their practice, they have come to acquiesce in that surrender, and have dispensed with habits of caution for which no one was likely to give them credit; while in their polemic they have as much resented the stringent demands for evidence, in which their opponents have been right, as the refusal to look at it when it is there, in which their opponents have been wrong. (pp. 4-5).

[The serious student of psychical phenomena] finds himself more or less in contact with advocates of new departments who ignore the weight of the presumption against them—who fail to see that it is from the recognized departments that the standard of evidence must be drawn, and that if speculation is to make good its right to outrun science, it will certainly not be by impatience of scientific canons. (p. 6).

Telepathy as a system of facts is what we have to study. Discussion of the nature of the novel faculty in itself, and apart from particular results, will be as far as possible avoided. That, if it exists, it has important relations to various very fundamental problems—metaphysical, psychological, possibly even physical—we can scarcely doubt. So far from the scientific study of man being a region whose boundaries are pretty well mapped out, and which only requires to be filled in with further detail by physiologists and psychologists, we may come to perceive that we are standing only on the threshold of a vast *terra incognita,* which must be humbly explored before we can even guess at its true extent, or appreciate its relation to the more familiar realms of knowledge. But such distant visions had better not be lingered over. Before the philosophical aspects of the subject can be profitably studied, its position as a real department of knowledge must be amply vindicated. This can only be done by a wide survey of evidence; the character of the present work will therefore be mainly evidential. (pp. 7-8).[4]

Lodge, in 1909, said:

An attitude of keen and critical inquiry must continually be maintained, and in that sense any amount of scepticism is not only legitimate but necessary.[5]

It is clear that the prominent investigators in the Society for Psychical Research who have been won over to the telepathic hypothesis appreciated to the full the general methods of science and yet regarded

[4] Gurney, Myers, and Podmore: Phantasms of the Living, vol. I. London, 1886.

[5] Lodge: Thought-Transference. *Forum,* 1909, 41:62.

the evidence for thought-transference as fulfilling the scientific criteria of proof. It is true that in agreement with the first Presidential Address delivered before the Society by Professor Sidgwick, on July 17, 1882, they have been at no time content with evidence of the best quality, but have always desired "a great deal more of it," since "the educated world, including many who have given much time and thought to this subject, are not yet convinced." "What I mean by *sufficient evidence,*" said Professor Sidgwick, "is evidence that will convince the scientific world." [6]

How far the scientific world is from being convinced is indicated by the fact that scientific men in general give the subject no attention in either their professional lecturing or their professional writing; even those scientific men into whose peculiar field of work telepathic phenomena properly fall either do not mention the subject at all in their text-books, or do so only to illustrate the factors involved in illusion or delusion. The writer recalls no text-book or other professional psychological literature (outside of M'Dougall's "Body and Mind," London, 2d ed., 1913, p. 349) which refers to thought-transference as a recognized phenomenon. It is evident that either scientific men are over-cautious, or the evidence is faulty.

Let us examine a few characteristics of the evidence.

The Creery Experiments.

In the "Phantasms of the Living" Edmund Gurney wrote:

> I have dwelt at some length on our series of trials with the members of the Creery family, as it is to those trials that we owe our own conviction of the possibility of genuine thought-transference between persons in a normal state.[7]

In the same place (p. 25) the able authors present a "Table Showing the Success Obtained when the Selected Object was Known to One or More of the Investigating Committee Only," in which are tabulated the trials made with the Creery sisters (Mary, Alice, Emily, and Maud) and a young maid-servant (Jane Dean), about twenty years of age, of the Creery household, at their home, in Buxton, at Cambridge, or in Dublin. The aggregate is 497 trials, the most probable number of successes is 27, the actual number of successes is, for the first guess 95, for the second 45, for either first or second 140. The "probability of obtaining by mere chance the amount of success which the first guesses gave,"

[6] *Proceedings S. P. R.*, 1882, 1:9.

[7] Vol. I, p. 29.

is recorded as .000,000,000,000,000,000,000,000,01. The eminent mathematician, Mr. F. Y. Edgeworth, who made the final calculations of probability said:

> These figures more impressively than any words proclaim the certainty that the recorded observations must have resulted either from *collusion* on the part of those concerned (the hypothesis of illusion being excluded by the simplicity of the experiments) or from *thought-transference* of the sort which the investigators vindicate.[8]

The authors point out that *collusion* between the members of the family will not account for the success when the objects guessed were known only to the investigators (at various times, Professor Barrett, Professor and Mrs. Sidgwick, Professor Balfour Stewart, Professor Alfred Hopkinson, Mr. and Mrs. F. W. H. Myers, and Edmund Gurney), and not to any member of the family;[9] and that, judging from the results, the conditions for success seemed to be no better when one or more members of the family also knew the object, name, number, or card to be guessed; unless it were Rev. Creery, whose integrity was above question, when they seemed decidedly better.[10]

The conditions of experiment may be seen from the following quotations from the published record:

> The second series of experiments (made on April 13, 1882) which we venture to think are unexceptionable, were made by Mr. Myers and Mr. Gurney, together with two ladies who were entire strangers to the family. None of the family knew what we had selected, the type of thing being told only to the child chosen to guess. The experimenters took every precaution in order that no indication, however slight, should reach the child. She was recalled by one of the experimenters and stood near the door with downcast eyes. In this way the following results were obtained.[11]

Out of 14 guesses on playing-cards, 9 complete guesses were made in the first guess (sometimes a second or third guess was allowed).

Another quotation from the record by Professor Balfour Stewart:

> After the first visit, one of my colleagues at Owens College remarked that it would be more conclusive if the thought-reader, instead of turning her face to the company, turned her face to the wall; and that was accordingly done on the second occasion. The percentage of success was about as large as in the first instance. In one case, while the thought-reader *remained* behind the door, a card was chosen.

[8] *Op. cit.*, p. 26.

[9] *Op. cit.*, pp. 22-3.

[10] *Op. cit.*, pp. 26-7.

[11] *Proceedings S. P. R.*, 1882, 1:22.

I chose the "ace of hearts," and the paper on which it was written down was handed round to the company. The thought-reader in a few moments called out, "Ace of hearts!"

These are all the experiments that I have to bring before you. While they cannot stand upon the same footing as those of Professor Barrett and his colleagues, they may be considered, I think, as corroborative of the experiments of these gentlemen. At any rate, if they are objected to, it will be necessary for our opponents to extend somewhat the area of untrustworthiness. I have no doubt when this operation is done again and again the objectors will get tired of it, and the laugh will then be turned against themselves.[12]

In the Committee's Second Report, they say:

We fully agree with our critics that both conscious and unconscious deception must be most carefully guarded against in all these cases. We shall continue to take all the precautions which experience suggests, and clearly to indicate in our Reports that we have taken them.[13]

And concerning the Creery experiments, Gurney later wrote:

This condition [when members of the investigating committee alone were agents] clearly makes it idle to represent the means by which the transferences took place as simply a trick which the members of the investigating committee failed to detect. The trick, if trick there was, must have been one in which they, or one of them, actively shared; the only alternative to collusion on their part being some piece of carelessness amounting almost to idiocy—such as uttering the required word aloud, or leaving the selected card exposed on the table.[14]

In the Appendix to the First Report, Professor Barrett said:

Only by wide and searching inquiry of this kind can any conception be formed of the pitfalls which beset the inquirer, arising mainly either from unconscious, involuntary actions, or from the extraordinary capacity there is in human nature for deception, often seemingly innocent, at other times resorted to for the sake of gaining notoriety. I confess I do not know how our vigilance would be increased, or our results become more trustworthy, by ability to diagnose any particular case, when experience has taught us to exercise habitual caution in all cases. The inquiry must ultimately resolve itself into a question of evidence, and demands the exercise of the faculty of careful observation, which a physicist is as likely to possess as a physiologist.[15]

Certain peculiarities in the experimental results did not pass unnoticed and were seized upon to support the statistical evidence or to throw light upon the nature of the thought-transference processes.

[12] *Op cit.*, p. 38.

[13] *Op. cit.*, p. 71.

[14] Phantasms of the Living, vol. I, p. 25.

[15] *Proceedings S.P.R.*, 1:47.

1. The proportion of correct guesses was not materially increased when members of the family were included among the agents; this was regarded as evidence that code was not used.

A large number of trials were also made in which the group of agents included one or more of the Creery family; and as bearing on the hypothesis of an ingenious family trick, it is worth noting that—except where Mr. Creery himself was included—the percentage of successes was, as a rule, not appreciably higher under these conditions than when the Committee alone were in the secret.[16]

2. Correct guesses were as frequent when the chances against success were incalculable as when they were greatly less.

In many trials, such as the guessing of fictitious names, made up by us on the spur of the moment, the chances against success were, of course, incalculable; yet, as will be seen by the following record taken from our last day's experimenting, these names were guessed with as much ease as cards, where the chances against success were far less.[17]

3. Many of the errors were approximate guesses; some part of the guess being correct, the guess of number being numerically close, or successive guesses approaching correctness:

One further evidential point should be noted. Supposing such a thing as a genuine faculty of thought-transference to exist, and to be capable, for example, of evoking in one mind the idea of a card on which other minds are concentrated, we might naturally expect that the card-pictures conveyed to the percipient would present various degrees of distinctness, and that there would be a considerable number of *approximate* guesses, as they might be given by a person who was allowed one fleeting glimpse at a card in an imperfect light. Such a person might often fail to name the card correctly, but his failures would be apt to be far more nearly right than those of another person who was simply guessing without any sort of guidance. This expectation was abundantly confirmed in our experiments. Thus, in a series of 32 trials, where only 5 first guesses were completely right, the *suit* was 14 times running named correctly on the first trial, and reiterated on the second. Knave was very frequently guessed as King and vice versa, the suit being given correctly. The number of pips named was in many cases only one off the right number, this sort of failure being specially frequent when the number was over six. Again the answer was often given, as it were, piecemeal—in two partially incorrect guesses—the pips or picture being rightly given at the first attempt, and the suit at the second; and in the same way with numbers of two figures, one of them would appear in the first guess and the other in the second. (pp. 27-8).[18]

4. An unaccountable fluctuation in the capacity was observed:

[16] Phantasms of the Living, p. 26.

[17] *Proceedings S. P. R.*, 1 : 24.

[18] Phantasms of the Living, vol. I.

The fluctuations in successes were very remarkable. Thus on one day, August 1st, when the guesser was outside the closed door, twenty-seven trials with cards gave not a single correct result; merely seven partial guesses, as eight of diamonds for seven of diamonds. Whereas on August 3d, apparently under precisely similar conditions, the guesser being outside the closed door and no sound of any kind permitted within the room where we, who knew the card, sat, ten trials gave two completely right and two almost right; and on August 4th, twenty-five trials under exactly the same conditions gave two completely right and two partially right. (p. 72).

Anxiety to secure success on the part of the subject is nearly always fatal and always prejudicial, hence the little trepidation that exists when set trials are made, or trials before strangers, tells most unfavorably. (p. 73).[19]

5. A regrettable decline in the capacity prevented a continuation of the research for the purpose of studying the laws operative in the phenomena.

It may be noted that the power of these (Creery) children, collectively or separately, gradually diminished during these months, so that at the end of 1882 they could not do, under the easiest conditions, what they could do under the most stringent in 1881. This gradual decline of power seemed quite independent of the tests applied, and resembled the disappearance of a transitory pathological condition, being the very opposite of what might have been expected from a growing proficiency in code-communication.[20]

It may perhaps be asked of us why we did not *exploiter* this remarkable family further. It was certainly our intention to do what we could in this direction, and by degrees to procure for our friends an opportunity of judging for themselves. . . . Had the faculty of whose existence we assured ourselves continued in full force, it would doubtless have been possible in time to bring the phenomena under the notice of a sufficient number of painstaking and impartial observers. (p. 29). . . . But the faculty did not continue in full force; on the contrary, the average of successes gradually declined, and the children regretfully acknowledged that their capacity and confidence were deserting them. The decline was equally observed even in the trials which they held amongst themselves; and it had nothing whatever to do with any increased stringency in the precautions adopted. No precautions, indeed, could be stricter than that confinement to our own investigating group of the knowledge of the idea to be transferred, which was, from the very first, a condition of the experiments on which we absolutely relied. The fact has just to be accepted, as an illustration of the fleeting character which seems to attach to this and other forms of abnormal sensitiveness. . . . The decline set in with their sense that the experiments had become matters of weighty importance to us, and of somewhat prolonged strain and tediousness to them. (p. 30).[21]

6. The phenomena of thought-transference were regarded as fairly common, and there is some evidence that it was curiously contagious:

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[19] *Proceedings S. P. R.*, 1:72-3.

[20] *Op. cit.*, 1:171.

[21] Phantasms of the Living, vol. I.

The Rev. A. M. Creery, in a "Note on Thought-Reading," said:

I may say that this faculty is not by any means confined to members of one family; it is much more general than we imagine. To verify this conclusion I invited two of a neighbor's children to join us in our experiments. On the first evening they were rather diffident, and did not succeed; on the second they improved, and on the third evening they were still better. Circumstances prevented them being able to continue their visits to us, but I saw enough to make me feel perfectly sure that had they persevered they would have been quite equal to our own circle in the faculty of thought-reading.[22]

Never before had such strong and apparently authentic evidence been presented to the world for the existence of thought-transference. The research was coöperative; the investigators were eminent men; responsibility for the proper conduct of experiments was distributed; the results were decisive in excluding chance. From the publication of the Committee's First Report criticism began pouring in from the press. Much of it was worthless; some of it doubtless assisted the investigators in improving their methods of experiment; but none of it shook the faith of the members of the Committee who had been won over to the telepathic hypothesis by this evidence. Some of these criticisms, which evidently charged the Society with unscientific methods, were answered in an address by the President of the Society, Professor Sidgwick:[23]

We admit, of course, that the majority of scientific experts still keep aloof from us, and that the agreement of experts is the final test of the establishment of truths;—indeed we may apply to the scientific world what an eminent statesman has said of the political world, that the main duty of a minority is to try to turn itself into a majority. But this is just what we hope to do; not so much by direct controversy, as by patiently and persistently endeavoring to apply to the obscure matters we are studying methods as analogous as circumstances allow to those by which scientific progress has been made in other departments. (p. 245).

This, then, is what we mean by a scientific spirit: that we approach the subject without prepossessions, but with a single-minded desire to bring within the realm of orderly and accepted knowledge what now appears as a chaos of individual beliefs. In saying that our *methods* are scientific we do not of course pretend to possess any technical knowledge or art, needing elaborate training. "Science," as an eminent naturalist has said, "is only organized common-sense"; and on ground so very new as most of that is on which we are trying to advance, the organization of common-sense, which we call scientific method, must necessarily be very rude and tentative. Indeed, the value to us of the scientific experts whom we are glad to count among our number depends much less on any technical knowledge or skill than on the general habit of mind—what I may call the "higher common-sense"—which their practice of scientific investigation has given to them; somewhat

[22] *Proceedings, S. P. R.*, 1:43-4; *cf.* also, *supra*, p. 29.

[23] Delivered July 18, 1883.

greater readiness and completeness in seeing considerations and adopting measures which, when once suggested, are not only intelligible, but even obvious, to the common-sense of mankind at large. (pp. 246-7).

We only refuse to admit them [the explanations of our opponents] where we find that the hypothesis manifestly will not fit the facts. (p. 248).

Before coming to our conclusion as to thought-transference we considered carefully the arguments brought forward for regarding cases of so-called "thought-reading" as due to involuntary indications apprehended through the ordinary senses; and we came to the conclusion that the ordinary experiments, where contact was allowed, could be explained by the hypothesis of unconscious sensibility to involuntary muscular pressure. Hence we have always attached special importance to experiments in which contact was excluded; with regard to which this particular hypothesis is clearly out of court. (p. 248).[24]

The Creery experiments completed, carefully prepared expositions of the research were published by the Committee in the *Nineteenth Century Magazine* [25] and in the "Phantasms of the Living." [26] Both publications called forth serious criticism. In the succeeding number of the *Nineteenth Century Magazine*,[27] appeared "A Note on 'Thought-Reading'" by Horatio Donkin, in which the critic said:

They [the Creery cases] differ in no way from the ordinary platform performances of the little "clairvoyantes" who from time to time have amused us both in the name of Second Sight, and in that of the humbler and honester one of conjuring. It is well known that a very simple code of signals will suffice to produce results much more startling than those we are discussing. . . . When the clairvoyante is not blindfolded other means of communication of course are possible, and in any case auditory signs other than words could be agreed upon quite unsuspected by the audience to be amused or deceived.

We have, therefore, an intelligible and admitted explanation which fully serves to cover all the facts in question. Such things are constantly done by collusion—it is a *vera causa.* It would be illogical to substitute for this a perfectly gratuitous hypothesis and an unknown agency. (p. 132).

The children were not blindfolded. (p. 132).

In most of the experiments there is no mention made of silence being preserved. (p. 133).

The mistake made by the servant in guessing the name "'Enry" for "Emily" is obviously significant, and an excellent example of an "undesigned coincidence." Surely it must lead almost every plain mind to the irresistible conclusion that a mistaken whisper or facial gesture played some part in the phenomenon. (p. 133).

The theory of collusion is moreover strongly countenanced by the fact of the mediums being children, who are always ready to join in any game of deception; and by the association with them of the servant-girl—a valuable fact, put-

[24] *Proceedings S. P. R.*, 1882-83, 1:245-8.

[25] June 1882, 11:890-901.

[26] 1886, vol. I, pp. 20-31.

[27] July 1882, 12:131-3.

ting out of court the assumption of any inherited special quality peculiar to the family, as an explanation, possibly plausible to some minds, of the alleged marvels. (p. 133).

The authors, indeed, say "though generally the object selected was shown to the members of the family present in the room, we were sometimes entirely alone." From the only rational point of view, that of scientific scepticism, and therefore with total disregard of the personal factor, this consideration seems in no way to invalidate the line of comment here taken. It is not clear to how many of the three observers the pronoun "we" in the above passage refers; but, at any rate, we miss entirely in the paper any specific quotation of results obtained in this latter set of circumstances.

But even if this evidence had been forthcoming, no mere *ipse dixit* on such a matter could for one moment be admitted. Reason would require us to entertain the great probability of mental bias in some at least of the observers, or to discredit the accuracy of their memory, rather than to allow that anything has been adduced in this account of what, to say the least, must be called superficially conducted experiments, to warrant a recognition of any novelty, or, by consequence, to stand in need of explanation by a theory of "Brain-waves." [28] (p. 133).[29]

Dr. G. Stanley Hall in his review of "Phantasms of the Living" pointed out that in spite of the authors' statement to the effect that the decline in the capacity of the Creery sisters for thought-transference "had nothing whatever to do with any increased stringency in the precautions adopted," the precautions did grow more stringent, and that there were facts which suggested either conscious or unconscious fraud:

That the precautions did grow more stringent there is abundant evidence. At first "*all* silently thought the name of the thing selected," after it had been written down and showed to the rest of the family. "The presence of the father seemed decidedly to increase the percentage of successes." At Cambridge, where the three elder sisters were isolated from their family, and where usually none of the sisters but the guesser knew the card selected, the successes were less numerous. Very significant is the series of thirty-two experiments where the sisters knew, and only the tops of their heads were visible to the guesser, and the suit was named correctly fourteen times running, with great positiveness and reiteration, while the card was right but five times. From twenty-seven experiments at Dublin, with the other sister knowing, the committee felt justified in saying that the presence and assistance of the sisters made no appreciable difference in the result, while at Cambridge only eight trials without and seven with the sisters knowing are given as the basis of a similar inference. From these random data the careful reader must infer that the effect of the presence of other members of the family

[28] At the close of the Committee's exposition there was a discussion of the "Brain-Wave" theory, and immediately following it was an article by the editor which consisted of excerpts from his articles to the *Spectator* on "Brain-Waves: A theory." (pp. 900-1).

[29] *Nineteenth Century Magazine*, 1882, 12: 131-3.

was far from sufficiently studied. Indeed the opposite conclusion from that of the committee is suggested. The latter expressly ascribed failure under strange conditions to diffidence, and aimed mainly to exclude only *conscious,* and underestimated unconscious, collusion, which the long previous practice at home must have made almost inevitable with a set of adolescent girls however honest or healthy. We should even like a more explicit statement as to how the other sisters were excluded from knowledge of the object selected, where they were, etc.

The methods of isolating the guesser are perhaps still less satisfactory. At first (Easter, 1881) the child was *sent* into the next room, and the name of the object was written and showed around. In April, 1882, the child was recalled by one of the experimenters, and movements in the room were excluded after the recall. Later a watcher was sent from the room with the child, and on being recalled the latter was placed with her eyes away from the company. Once at least she was isolated behind the door, at Dublin "behind an opaque curtain" (although we have found an hypnotically sensitized subject who could read large letters in sunlight through five thicknesses of cotton sheeting). In this most vital respect also there was not only no rigorous control and no systematic method, but only the first rude and irregular preludes and approaches toward it. (p. 130).

Again we cannot forbear asking if *every* experiment, without exception, in Mr. Barrett's session of Easter, 1881, to say nothing of Mr. Stewart's sessions, was recorded; and if the results were all noted on the spot and the conditions and descriptions taken at the time and place. The experience of the Seybert commission in these respects, especially the latter, as well as the writer's own experience with the untrustworthiness of memory, even for an hour, where such complex conditions and interests are involved, is sufficient to justify this query. The report of July, 1882, is not explicit on these matters, and, from a careful scrutiny of it, it appears at least doubtful. When was the paragraph in quotation marks beginning page 21 written, and when the very general description of conditions on page 20? If Mr. Barrett himself made or dictated these notes on the spot, how could he, as the only person present besides the family, possibly so secure himself against all the manifold and subtle sources of fallacy in observation as to be warranted in calling his tests on this occasion "absolutely unexceptionable and conclusive" as he recklessly does? Next in importance to the method of experimenting in so hazardous a field is the way of making the protocol and working up the final form of the report. Either this or the accuracy of observation, or more probably with but one observer, however good, against so many possibilities of error, both must suffer. If either of these surmises is correct, it bears against the statement that the girls gradually lost their power from any cause but increased precautions on the part of the experimenters. (pp. 130-1).

In the study of hyperaesthesic states we now begin to realize the possibilities of our sensory organism, and how greatly the limits of just observability vary at different times and states and in different persons, and how it responds physically to the remotest and faintest cosmic influences. Hearing, *e. g.,* which is known to vary exceedingly among people whose auditory sensitiveness passes for normal, the writer has carefully tested in many persons. Two individuals were selected for exceptional acuteness in this sense, and the following simple code devised by the writer, which, though repeatedly tried in critical companies, has never been detected, and with results that impressed many as genuinely telepathic. Pulsa-

tions, felt subjectively by the percipient, and easily counted by the agent, either by movement of the toe if one leg was crossed on the other, or directly seen in the aorta, or in vibrations of hairs or neck-ribbons, were the basis. The faintest respiratory noises through the nose, or even mouth, of the agent, were made to coincide with and accent certain pulsations of the percipient. To make these modified breathings so distinct as to be clearly heard at a distance by the normal sensitive, yet so faint as to be inaudible even if listened for by spectators often nearer than the agent, was the art of the latter. If a number was selected by some one present the agent caught the rhythm of the percipient's pulse, and could hold it for some time if blindfolded, or then see it in the toe occasionally, and gave a very faint sniff coinciding with a pulsation, and from this, as zero, the percipient counted till the next sniff for the first digit, then till the next sniff for the second digit, and so on. For cards, first the suit and then the card, were counted off. The alphabet went more slowly, but a series of diagrams, from which the selection by the company was made which had been memorized and numbered, was the greatest success. When the percipient's ears were stopped, coughs, jars on the table or floor, etc., helped us out. This trivial code, however, essentially depended ultimately for the absolute security it generally possessed on the fact that the percipient could hear more acutely than any one present, and when that is the case a telepathy not outside the ordinary channels of sense is possible. We have no data whatever for believing that the ear can hear and distinguish muscle or pencil sounds in making different letters, hear whispers through a couple of doors, etc., but neither have we adequate data for judging how far these, or even less desperate possibilities in the field of vision, would need to be stretched to account for some of the Creery phenomena. (p. 131-2).

We have dwelt with some detail on the Creery phenomena because Mr. Gurney, Mr. Myers and Professor Barrett, the most active workers and probably the best observers in the English Society, spent so much time on them, and because the former expressly states that "it is to those trials that we owe our own convictions of the possibility of genuine thought-transference." Hence a bias certainly existed in all subsequent experiments. The precautions grew more strict, and probably, as we infer, the record grew fuller and more exact, and what is called a decline in telepathic power we should interpret from between the lines of the record as an approach to the heart of the mystery, which ought to encourage unbiased investigators to press onward to a beckoning goal. The girls grew discouraged and did not succeed with each other, it is said. This is natural, as interest in their performances diminished. But it is strange that this decline should coincide step by step with closer study of them, and still more so that all the girls should lose this marvelous power *simultaneously!* (pp. 133-4).[30]

In the *Journal*[31] of the Society appears the following note:

It will be remembered that the earliest experiments in Thought-transference described in the Society's *Proceedings* were made with some sisters of the name of Creery; and that though stress was never laid on any trials where a chance

[30] *American Journal of Psychology,* 1888, 1: 128 ff.

[31] October 1887, 3: 164.

of collusion was afforded by one or more of the sisters sharing in the "agency," nevertheless some results obtained under such conditions were included in the records. In a series of experiments recently made at Cambridge, two of the sisters, acting as "agent" and "percipient," were detected in the use of a code of signals; and a third has confessed to a certain amount of signaling in the earlier series to which I have referred. This fact throws discredit on the results of all former trials conducted under similar conditions. How far the proved willingness to deceive can be held to affect the experiments on which we relied, where collusion was excluded, must of course depend on the degree of stringency of the precautions taken against trickery of other sorts—as to which every reader will form his own opinion. A further notice of the facts here briefly stated will be published in the *Proceedings*.

And in the *Proceedings*[32] appears a two-page article entitled a "Note Relating to Some of the Published Experiments in Thought-Transference," from which we quote:

The code was as follows:—When the two sisters were in sight of one another, the signals used were a slight upward look for hearts, downwards for diamonds, to the right for spades, and to the left for clubs. Further, the right hand put up to the face meant king, the left hand to the face meant queen, and knave was indicated by crossing the arms. It is doubtful whether there were any signs for other cards. We failed to make any out clearly. A table showing the degree of success in guessing each card suggests that there were signs for 10 and ace, but that they were either only used occasionally or used with poor success.

In experiments in which a screen was placed between the two sisters, so that they could not see each other, auditory signs were used to indicate suits. A scraping with the feet on the carpet meant hearts, and sighing, coughing, sneezing or yawning meant diamonds. If there were signs to distinguish between the black suits they were—like the signs for 10 and ace in the visual code—sparingly used or often unsuccessful.

The sisters are naturally very restless, which made the movements above described less obvious than they would otherwise have been. As soon as some clue to the code used had been obtained, Mr. Gurney and Mrs. Sidgwick, and sometimes Professor Sidgwick, set themselves to guess the card (which they took care should be unknown to them) from the signals, secretly recording their guesses. Their success afforded a complete proof of the use of the signals. . . .

The account which was given as to the earlier experiments conducted under similar conditions, is that signals were very rarely used; and not on specially successful occasions, but on occasions of failure, when it was feared that visitors would be disappointed.

The publication of the note in the *Journal* called forth a letter from the Rev. A. M. Creery, in which he said:

Dear Sir,—The announcement which appears in the last *Journal* that my daughters were detected using a code of signals in some thought-transference ex-

[32] *Proceedings S. P. R.*, 1888-89, 5: 269-70.

periments at Cambridge, has given me intense pain; and I have no desire to excuse their misconduct, nor to extenuate their guilt, for which they now grieve quite as much as I do. But I do not believe that signs, signals, and hints of any kind were used in the earlier experiments. It would, of course, be impossible to say that a sign was never used in the thousands of experiments that were made, not only before scientific and literary men, but in numerous drawing-rooms as an evening amusement, during the two or three years in which we were interested in the matter, though I was never aware of it; but that anything like a code of signals was ever in use during the early experiments which which I had anything to do, I do not believe. (p. 175).

. . . And yet, during all that time I never heard they were suspected of using signals. Had they been in the habit of doing so, we might have expected to find them improving in their guessing, according as "the code" became more perfect by practice; but the very reverse was the case. And as I found, after the early part of 1882, that their faculties of percipience were gradually deteriorating, I resolved to give up the experiments; and it was contrary to my advice and wish that they were recommenced after a lapse of five years, knowing the power of the temptation, which in somewhat kindred matters has proved almost universally fatal, to simulate by tricks what formerly came spontaneously and naturally.

The last word that I shall say on this matter is this: that if the scientific investigators, all of whom afterwards became prominent members of the "Society for Psychical Research," could have been deceived by a few children practising a "code of signals," their keenness of vision, and their faculty of "continuous observation," are less than I could have imagined. (p. 176).[33]

This blow to the evidence the Committee did not appear to regard as fatal, since occasional resumés [34] of evidence for thought-transference have since appeared from the pens of the members of the Committee or of the Society which include the Creery cases. They appear to have regarded the internal evidence of precautions against collusion, in the published records, as sufficient to indicate that, for example, when the Committee alone acted as agents, a code would have been useless.

Yet, when one examines the internal evidence, much that was used to support the telepathic hypothesis is equally good support for undiscovered signaling—even in the data from which the "agency" of the sisters was supposed to be excluded. Let us examine briefly and *seriatim* the list [35] of peculiarities in the experimental results which observers remarked:

1. Since the proportion of correct guesses when the members of the family were included among the agents was not greater than when

[33] *Journal S. P. R.*, November 1887, 3: 175-6.

[34] For example, Sir W. F. Barrett: Psychical Research. London, 1911, pp. 53 ff. But the Creery series have been avoided by Podmore, Lodge and Thomas.

[35] *Supra*, pp. 465 ff.

they were excluded, like causes were probably operative. Some signaling, one of the girls confessed, had taken place; can one be sure that the members of the family were really excluded from "agency" when the investigators supposed that they were, or that there has been no error in the records, or procedure? Were the cards new and unmarked? Were spectacle reflections avoided?

2. Correct guesses were as frequent when the chances against success were incalculable as when they were greatly less. That is, whether guesses were made upon fancy names or upon suits of cards. Take, for illustration, the name "Alfred Henderson" which Mary Creery guessed at the first trial; the chance of getting the first letter right is 1/36; of getting *Al* right, is $1/36 \times 1/36$; of getting the whole name, is $1/36^{15}$ or .000,000,000,000,000,000,000,004,5; while the chance of getting a suit of cards right is 1/4 or .25. Now on the assumption that a capacity for thought-transference operated with such force that, in the long run, about one-third of the mental content is transferred to the percipient, we would expect in right guesses about 33% on names, and about 50% on the suit of cards.[36] On the assumption of a code, we would expect about 100% right when the conditions were favorable for the use of the code, and none right on names, and but 25% on suit, when the conditions precluded its use. The absence of gradation in the proportion of correct guesses in accordance with the theoretical chances, strongly supports the hypothesis of code.

3. The approximate guessing constitutes *prima facie* evidence of code. The run of fourteen correct guesses on suit occurred at Cambridge, August 2d, 1882, when the two sisters who were included among the agents were screened from the third sister, who was acting as percipient, in such a way that the tops of their heads only were visible to the percipient.[37] The visual code discovered consisted, for suit, in

[36] Out of 100 guesses on suit, 25 would be right by chance; one-third of the remainder (75) by thought-transference would be 25; making the total of right guesses 50.

[37] A remarkable instance of this partial perception of the thing selected occurred on August 2d. On this occasion all the Committee were present; two of the sisters of the guesser were also in the room, and knew the card selected; they were, however, so placed that (though they were completely in our view) only the tops of their heads were visible to the guesser, and they remained quite motionless and silent throughout the experiments. Out of 32 experiments with cards, 5 were guessed completely right at the first attempt, and in addition 20 were partially right. *Fourteen times running* the suit was named correctly on the first trial, and reiterated on the second; not only was no indication whatever given to show that

angular disposition of the head; for number, in disposition of the hands or arms. Code for suit could be used; code for number could not. The fact that the number of pips in wrong guesses was most often wrong by but one digit when the number was over six points unmistakably to the use of signal-code under conditions of difficulty. A mistake in count, or failure to see or hear the signals distinctly, would result in such errors. This evidence is supported by the fact that the same sort of error is to be found in the guesses on two-place numbers (66 for 67, 54 and 56 for 55, 71 for 81, 16 for 18, 26 for 29, 22 for 21, etc.), where the supposed cause (an indefinite mental imagery of the face of a playing-card) of the errors in guessing pips could not be operative. The progressive approximation of successive guesses to correctness is also a characteristic of code-reading.

4. The unaccountable fluctuation in the proportion of correct guesses is thoroughly consistent with signaling which would work under certain conditions only. One would expect such fluctuations, under the varying conditions of the experiments, more readily of code-reading than of thought-reading.

5. The decline in the capacity is readily interpreted as a result of the growing disinclination of the pastor's family for perpetrating what had at length come to appear to them as a serious imposture, but what had been readily entered into in the beginning in a spirit of innocent mischief. The seriousness of the enterprise may have been partly impressed upon them by the increased vigilance of the Committee and partly by learning something of the scientific import of the investigation. In order to discourage further experimentation they would naturally display less power in the trials which they held amongst themselves. This interpretation would account for the apparent independence of the decline and "any increased stringency in precautions adopted"; it would account for the curious fact that the power of the children declined "collectively and separately"; and it would not involve any curious characteristics of a "pathological condition" or an "abnormal sensitiveness."

The decline set in with their sense that the experiments had become matters of weighty importance to us, and of somewhat prolonged strain and tediousness to them.[38]

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the suit was rightly named, but our impassive countenances and the solitary word "No," failed to displace from the percipient's mind the correct impression of the suit. The chances against success in naming the suit rightly in any one case are of course 3 to 1, but the chances against being right fourteen times *consecutively* are 4,782,969 to one.—*Proceedings S. P. R.*, 1:74.

[38] Phantasms of the Living, vol. I, p. 30.

6. The hypothesis of code is remarkably consistent with the otherwise curious fact that the power to augment correct guesses decisively beyond chance was contagious. Jane Dean was quite as successful as the Creery sisters in whose home she lived in the capacity of maid-servant. And the neighbor's children who were called in to join the family in their experiments, and who were diffident and unsuccessful on the first evening, improved so much on the second and still more on the third that Mr. Creery felt sure that had they been able to continue they would have equaled the members of his own circle.

And there is one more bit of internal evidence in the published results that may be mentioned in support of the hypothesis of code.

> On one or two occasions it seemed of advantage to obtain vivid simultaneous realization of the desired word on the part of all sitters; which is most easily effected if some one slowly and gently claps time, and all mentally summon up the word with the beats.[39]

If this device originated with one of the sisters and the clapping was performed by one of them, the process of thought-transference would be entirely consistent with the later confession.

The internal evidence here adduced, together with the fact that those successful experiments have not been repeated in the thirty-five years of experimental work on the problem by the Society for Psychical Research and other responsible investigators, will probably be sufficient to convince the reader that the Committee must somehow be wrong in its claim that it excluded from an assigned portion of its experiments the use of code.

The Smith-Blackburn Experiments.

The second important series of experiments carried out by the Committee were made with Mr. Douglas Blackburn, Editor of the *Brightonian,* and Mr. G. A. Smith, a young mesmerist, both of Brighton.

Attention was drawn to these men by a letter published in *Light,* from which Professor Barrett presented in the Appendix to the First Report the following quotation:

> The way Mr. Smith conducts his experiments is this: He places himself *en rapport* with myself by taking my hands; and a strong concentration of will and mental vision on my part has enabled him to read my thoughts with an accuracy that approaches the miraculous. Not only can he, with slight hesitation, read numbers, words, and even whole sentences which I alone have seen, but the sympathy

[39] *Proceedings S. P. R.,* 1 : 29.

between us has been developed to such a degree that he rarely fails to experience the taste of any liquid or solid I choose to imagine. He has named, described, or discovered small articles he has never seen when they have been concealed by me in the most unusual places, and on two occasions he has successfully described portions of a scene which I either imagined or actually saw.[40]

DOUGLAS BLACKBURN, Editor of *Brightonian.*

Mr. Blackburn is represented in the Second Report,[41] as an associate of the Society, and "a very painstaking and accurate observer." The second paragraph of the report of the "Brighton Experiments" follows:

We entered into correspondence with Mr. Blackburn, who thereupon took the trouble to send us a paper recording in detail his experiments with Mr. Smith. These statements appeared to be so carefully made that two of our number, Mr. Myers and Mr. Gurney (Mr. Barrett being unable to go at the time), arranged to pay a visit to Brighton personally to investigate the joint experiments of Mr. Blackburn and Mr. Smith. These gentlemen most obligingly placed themselves at our service, and a series of trials were made in our own lodgings at Brighton. The results of these trials give us the most important and valuable insight into the manner of the mental transfer of a picture which we have yet obtained. (pp. 78-9).

Experiments were begun December 3, 1882:

S. was blindfolded at his own wish to aid in concentration, and during the experiment sat with his back turned to the experimenters.

B. holds S.'s hand, and asks him to name a color, written down by one of us and shown to B. It is needless to say the strictest silence was preserved during each experiment.

[Eight experiments on naming of colors.]

After a rest *numbers* were then tried in the same way.

	Number selected	Answer
Expt. *9*	35	34
" *10*	48	58
" *11*	7	7

Several trials of colors and numbers were now made with S. and B. in separate rooms, which failed. *Names* were next tried, written down and shown to B., who then took S.'s hand as before. There was, as usual, no sound nor movement of the lips on the part of any one.

	Name chosen	Answer
Expt. *12*	Barnard	Harland, Barnard
" *14*	Johnson	Jobson, Johnson

[40] *Proceedings S. P. R.*, 1882-83, 1 : 63.

[41] *Ibid.*, pp. 70-97.

Two names were then tried without any contact, as follows:—

	Name chosen	Answer
Expt. *16*	Hobhouse	Hunter
" *17*	Black	Drake, Blake

Contact between S. and B. was now resumed by our express desire, as the increased effort of concentration, needed when there was no contact, brought on neuralgia in B.

	Name chosen	Answer
Expt. *18*	Queen Anne	Queechy, Queen
" *19*	Wissenschaft	Wissie, Wissenaft

As B. was ignorant of German, he mentally represented the word "Wissenschaft" in English fashion. (pp. 79-80).

We next drew a series of diagrams of a simple geometrical kind, which were placed behind S., so that B. could see them. S. described them in each case correctly, except that he generally reversed them, seeing the upper side of the diagram downward, the right hand side to the left, etc.

Next day (December 4th) we varied this experiment, thus:—

One of us, completely out of sight of S., drew some figure at random, the figure being of such a character that its shape could not be easily conveyed in words; this was done in order to meet the assumption that some code—such as the Morse alphabet—was used by S. and B. The figure drawn by us was then shown to B. for a few moments,—S. being seated all the time with his back to us and blindfolded, in a distant part of the same room, and subsequently in an adjoining room.

B. looked at the figure drawn; then held S.'s hand for a while; then released it. *After being released,* S. (who *remained* blindfolded) drew the impression of a figure which he had received. It was generally about as like the original as a child's blindfold drawing of a pig is like a pig; that is to say, it was a scrawl, but recognizable as intended to represent the original figure. In no case was there the smallest possibility that S. could have seen the original figure; and in no case did B. touch S. even in the slightest manner, while the figure was being drawn.

In one case, No. 6 in the series, the copy may be said to be as exact as S. could have drawn it blindfold if he had previously seen the original. The figures were not *reversed* on this day, as they had been on the previous one.

The whole series of figures (nine in number) are given in the accompanying plates, which are engraved from photographic reproductions, *on the wood blocks,* of the original drawings. (pp. 80-2).

In its Third Report [42] the Committee gives the results of experiments performed in January, 1883, in the rooms of the Society in London. Great care was taken to record minutely the conditions of each experiment.

[42] *Ibid.,* pp. 161-215.

Our *modus operandi* is as follows: The percipient, Mr. Smith, is seated blindfolded at a table in our own room; a paper and pencil are within his reach, and a member of the Committee is seated by his side. Another member of the Committee leaves the room, and outside the closed door draws some figure at random. Mr. Blackburn, who, so far, has remained in the room with Mr. Smith, is now called out, and the door closed; the drawing is then held before him for a few seconds, till its impression is stamped upon his mind. Then, closing his eyes, Mr. Blackburn is led back into the room and placed standing or sitting behind Mr. Smith, at a distance of some two feet from him. A brief period of intense mental concentration on Mr. Blackburn's part now follows. Presently, Mr. Smith takes up the pencil amidst the unbroken and absolute silence of all present, and attempts to reproduce on paper the impression he has gained. He is allowed to do as he pleases as regards the bandage round his eyes; sometimes he pulls it down before he begins to draw, but if the figures be not distinctly present to his mind, he prefers to let it remain on, and draws fragments of the figure as they are perceived. (p. 162).

Only eight experiments, in the reproduction of designs, out of a total of 37 can be put down as unsuccessful. In the first four experiments contact was permitted,

. . . as a supposed aid to Mr. Smith in visualizing Mr. Blackburn's mental picture. We, however, could allow no exception to our cardinal axiom on this subject, that no experiment where contact of any sort is allowed can be decisive; and though in the present instance the drawings were of such an irregular character that their description would have been extremely difficult to convey by imperceptible tracing or by any subtle code of pressure-signs, yet, assuming Mr. Blackburn and Mr. Smith to have been in collusion, the hypothesis was at least conceivable. Accordingly, we requested Mr. Blackburn to dispense altogether with the preliminary contact; and it must be understood that all the rest of the successful drawings (with the exception of two, not here reproduced, and of Fig. 13 as explained below) were done without any contact whatever, in the manner already indicated on p. 162. (p. 163).

We have now to consider whether it was possible that any information of the character of the designs drawn could have reached Smith through the ordinary avenues of sense. Of the five recognized gateways of knowledge, four—tasting, smelling, touch, and sight—were excluded by the conditions of the experiment. There remains the sense of hearing, which was but partially interfered with by the bandage over the eyes and ears. But the information can certainly not have been conveyed by speech; our ears were as near to Mr. Blackburn as Mr. Smith's, and our eyes would have caught the slightest movement of his lips.

There remains the hypothesis of a code, consisting of audible signals other than oral speech; and it would, no doubt, be an exaggeration to affirm that the possibility of such signals was absolutely excluded. We shall endeavor so to vary the conditions of subsequent experiments as to exclude this hypothesis completely; at present we will only point out the very great improbabilities which it involves, quite independently of our reliance on the integrity of Mr. Blackburn and Mr. Smith, which nothing has occurred to shake in the slightest degree. (p. 164).

It is probably no exaggeration to say that several scores, if not hundreds, of

precise signs would be required to convey an idea as exact as that implied in many of Mr. Smith's representations. But in our experiments what sort of range existed for this mode of communication? The material for possible signs appears to be reduced to shuffling on the carpet, coughing, and modes of breathing. Anything distinctly unusual in any of these directions must inevitably have been noticed; and since our attention, during this part of the experiment, was of course concentrated on the relation between Mr. Blackburn and Mr. Smith we are at a loss to conceive how any signaling, sufficient in amount to convey the required ideas, could have passed undetected. Furthermore, it must be observed that the reproductions were not made in a tentative, hesitating manner as if waiting for signals; but deliberately and continuously as if copying a drawing that is seen. Moreover, in almost every instance the *proportions* of the different parts of the original figure were reproduced more accurately than were its more easily describable details. However, with the view of removing all doubts that might arise as to possible auditory communications, we on one occasion stopped Mr. Smith's ears with putty, then tied a bandage round his eyes and ears, then fastened a bolster-case over the head, and over all threw a blanket which enveloped his entire head and trunk. No. 22 was now drawn by one of us, and shown outside the room to Mr. Blackburn, who on his return sat behind Mr. Smith, and in no contact with him whatever, and as perfectly still as it is possible for a human being to sit who is not concentrating his attention on keeping motionless to the exclusion of every other object. In a few minutes Mr. Smith took up the pencil and gave the successive reproductions shown below [Fig. 7].

To profit by a code in this case, Mr. Smith would have had to extract the putty from his ears unobserved by us, (an action the possibility of which the heavy swarthings rendered just conceivable,) and then, still smothered in bolster-case and blanket, to detect periodic variations in Mr. Blackburn's breathing imperceptible to us; to identify them as proceedings from Mr. Blackburn, and to interpret them into a description of the figure given below. This hypothesis seems to us an extreme one, but, as we have already said, we intend to meet it by yet further varying and narrowing the conditions of future experiments. (p. 165).[43]

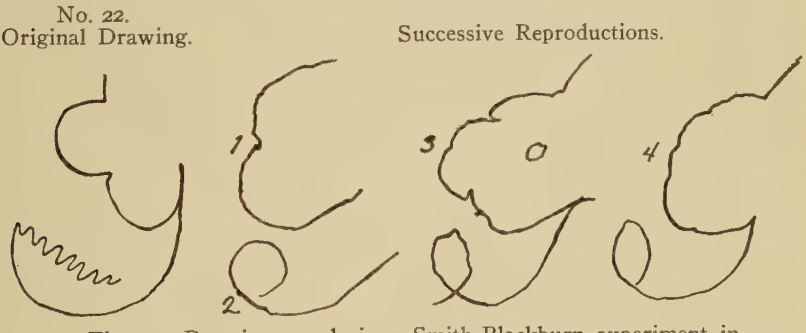

Fig. 7.— Drawings made in a Smith-Blackburn experiment in Thought-Transference.

[43] *Proceedings S. P. R.*, 1882-83, 1 : 164-5.

Dr. G. Stanley Hall in his review of "Phantasms of the Living" offered the following criticisms of this series:

With the record of these seventeen reproductions, without contact, of the most unconventional diagrams we confess ourselves more deeply impressed than with any other work of the society, especially the remarkable No. 22, reproduced with the ears of the percipient stopped with putty, and wraps enveloping the entire upper part of his body. We can but wish, however, there had been more of these, and consecutively, and that while they were about it the committee had isolated the lower part of Mr. Smith's body from all sensation of jars, and carried, rolled or swung the agent into the room to exclude the possibility of a code by steps which an American exhibitor has brought to an incredible degree of development, and also tested the amount of reduction of acuteness of audition in each ear of Mr. Smith, or at least of themselves, by putty, and taken precautions to make sure that all light from the floor was excluded from Mr. Smith's eyes. We have practiced with some success a toe-code, a part of which is by minimal movements of the great toe within a thin shoe, the latter not moving at all, and a part, for complex figures, involving slight movement of the toe of the shoe, which we should think would only be facilitated by the conditions of No. 22 with overhanging wraps to shield it from the committee. Moreover, it is just these larger general forms and relations of parts without finer details that are best transmitted thus, while the latter, as the committee note, are absent. It seems worse than Mephistophelean—indeed we wish we were freer from the fear that it is so in very truth—to even suggest, in place of tension toward transcendental entities, slight practiced movements of the big toe. . . . The first four figures where contact was allowed are certainly much better reproduced than any other four in the series, and in Fig. 13 an entire change of the percipient's conception of the model was caused by contact, though unfortunately it is not stated whether Blackburn touched Smith again after he had corrected his conception of the original, and before it was correctly reproduced, or whether the second reproduction coincided with Blackburn's memory of it. Again in Fig. 6, contact suggested a remote reproduction. Suggestive, too, is the circumstance that after contact was excluded Mr. Blackburn explains so many deviations in the copy into greater conformity with the originals by mistakes in his own conception, which he had done in no previous case in this or the earlier series. A cross in one would grow too large in his mind's eye against his will; he had "not precisely remembered" another; forgot the eyes in another; focused on one part only in another, and imagined curves in the opposite direction in another. The tests to account for the mental inversion of objects, which strikes us as just the thing, were only forty-two in number, the result being that eighty-seven per cent of the answers gave the direction in which a vertical and only thirty-seven per cent gave that in which a horizontal arrow pointed. May we add that we have found the same advantage of perpendiculars in the toe-code, on account of the relative difficulty of lateral movement? This is doubtless entirely irrelevant, for it is expressly stated that Smith sees in his mental shrine the image of a white arrow on a dark ground and instantly detected the change when a white arrow on a red ground was substituted for an ink-drawn one. This aside, however, we deem the evidence considerable that after contact was given up either a new and less practiced or more difficult code (conscious or unconscious) was used, or else that the

unknown telepathic forces were obliged to find and deepen other lines of least resistance.[44]

A number of years later, Professor Minot made the following comments on this series:

A few of the drawings were of a more complicated character, notably those in the trials conducted with Messrs. Smith and Blackburn under the auspices of the English Society for Psychical Research. In this special case it is possible to explain the apparent thought-transference by the hypothesis of intentional collusion between the performers, at least in great part. That the diagrams can be reproduced from descriptions, I have ascertained by trial, and that in many of the Smith-Blackburn experiments there was opportunity for signaling cannot be denied; therefore it is quite possible that in this instance the percipient was told by his confederate what to draw. That mere watching is not enough to guard against possible deception by expert collusionists is well known. (p. 225).

The most impressive tests of which we have accounts are those the value of which depends upon the attentive watchfulness of the investigators. Now, we have not only the proof from Mr. Davey [45] that attention is not a really sufficient guard, and that the essential incidents are unnoticed, but also the proof from the collapse of the Creery case, that the English observers have been inattentive and unobservant in precisely the manner Mr. Davey's slate-writing tests render probable. In relying to the extent they have done upon watching with sustained attention, the English experimenters have again failed to come up to a scientific standard, and they leave fraud an open explanation for many of their results. (p. 226).

After a thorough examination of the evidence adduced, I am brought to the conclusion that thought-transference, even as a hypothetical explanation, is a superfluous conception. (p. 227).[46]

In a reply to Professor Minot, Mr. Frank Podmore pointed out that the Committee showed its astuteness in detecting the signaling of the Creery sisters in the later experiments, and that signaling would be useless to the sisters, when the investigators alone knew the object guessed. He continued:

It is to be regretted that Mr. Minot's study of the subject ceased apparently five or six years ago, and that he almost wholly ignores the great mass of evidence accumulated since 1888. (p. 333).

His [Minot's] energies have been so unhappily diverted to the demonstration of faults which do not exist that he has never touched at all upon the weakest spot in our experimental evidence. For whilst no fully informed critic would assert that the experiments on which we rely as establishing thought-transference are due to either chance or fraud, such an one could plausibly maintain that at

[44] *American Journal of Psychology*, 1888, 1:135-6.

[45] *Vide, Proceedings S. P. R.*, 4:405-495; 8:256-9.

[46] Minot: The psychical comedy. *North American Review*, February 1895, 160:217-230.

least a great part of them may be explained as the result of information unconsciously conveyed by normal channels from agent to percipient, and no candid investigator would meet such a criticism with a direct negative. This indeed is the crux of the whole inquiry. (p. 336).[47]

In 1911, there appeared in the *London Daily News* of September 1st over the signature of Douglas Blackburn a communication from which the following excerpts are made:

For nearly thirty years the telepathic experiments conducted by Mr. G. A. Smith and myself have been accepted and cited as the basic evidences of the truth of Thought Transference.

Your correspondent "Inquirer" is one of many who have pointed to them as a conclusive reply to modern sceptics. The weight attached to those experiments was given by their publication in the first volume of the *Proceedings of the Society for Psychical Research,* vouched for by Messrs. F. W. H. Myers, Edmund Gurney, Frank Podmore, and later and inferentially by Professor Henry Sidgwick, Professor Romanes, and others of equal intellectual eminence. They were the first scientifically conducted and attested experiments in Thought Transference, and later were imitated and reproduced by "sensitives" all the world over.

I am the sole survivor of that group of experimentalists, and as no harm can be done to anyone, but possible good to the cause of truth, I, with mingled feelings of regret and satisfaction, now declare that the whole of those alleged experiments were bogies, and originated in the honest desire of two youths to show how easily men of scientific mind and training could be deceived when seeking for evidence in support of a theory they were wishful to establish.

And here let me say that I make this avowal in no boastful spirit. Within three months of our acquaintance with the leading members of the Society for Psychical Research, Mr. Smith and myself heartily regretted that these personally charming and scientifically distinguished men should have been victimized; but it was too late to recant. We did the next best thing. We stood aside and watched with amazement the astounding spread of the fire we had in a spirit of mischief lighted.

The genesis of the matter was in this wise. In the late seventies and early eighties a wave of so-called occultism passed over England. Public interest became absorbed in the varied alleged phenomena of Spiritualism, Mesmerism, and thought-reading; "professors" of the various branches abounded, and Brighton, where I was editing a weekly journal, became a happy hunting ground for mediums of every kind. I had started an exposure campaign, and had been rather successful. My great score was being the first to detect the secret of Irving Bishop's thought-reading. In 1882 I encountered Mr. G. A. Smith, a youth of 19, whom I found giving a mesmeric entertainment. Scenting a fraud, I proceeded to investigate, made his acquaintance, and very soon realized that I had discovered a genius in his line. He has since been well known as a powerful hypnotist. He was also the most ingenious conjurer I have met outside the profession. He had the

[47] Podmore: What psychical research has accomplished. *North American Review,* March 1895, 160:330-344.

versatility of an Edison in devising new tricks and improving on old ones. We entered into a compact to "show up" some of the then flourishing professors of occultism, and began by practicing thought-reading. Within a month we were astonishing Brighton at bazaars and kindred charity entertainments, and enjoyed a great vogue. One of our exhibitions was described very fully and enthusiastically in *Light*, the spiritualistic paper, and on the strength of that the Messrs. Myers, Gurney, and Podmore called on us and asked for a private demonstration. As we had made a strict rule never to take payment for our exhibitions, we were accepted by the Society as private unpaid demonstators, and as such remained during the long series of séances.

It is but right to explain that at this period neither of us knew or realized the scientific standing and earnest motive of the gentlemen who had approached us. We saw in them only a superior type of the spiritualistic cranks by whom we were daily pestered. Our first private séance was accepted so unhesitatingly, and the lack of reasonable precautions on the part of the "investigators" was so marked, that Smith and I were genuinely amused, and felt it our duty to show how utterly incompetent were these "scientific investigators." Our plan was to bamboozle them thoroughly, then let the world know the value of scientific research. It was the vanity of the schoolboy who catches a master tripping.

A description of the codes and methods of communication invented and employed by us to establish telepathic rapport would need more space than could be spared. Suffice it that, thanks to the ingenuity of Smith, they became marvelously complete. They grew with the demands upon them.

Starting with a crude set of signals produced by the jingling of pince-nez, sleeve-link, long and short breathing, and even blowing, they developed to a degree little short of marvelous. To this day no conjurer has succeeded in approaching our great feat, by which Smith, scientifically blindfolded, deafened, and muffled in two blankets, reproduced in detail an irregular figure drawn by Mr. Myers, and seen only by him and me.

The value of a contribution such as this should lie not so much in describing the machinery as in pointing out how and where these investigators failed, so that future investigators may avoid their mistakes.

I say boldly that Messrs. Myers and Gurney were too anxious to get corroboration of their theories to hold the balance impartially. Again and again they gave the benefit of the doubt to experiments that were failures. They allowed us to impose our own conditions, accepted without demur our explanations of failure, and, in short, exhibited a complaisance and confidence which, however complimentary to us, was scarcely consonant with a strict investigation on behalf of the public.

That this same slackness characterized their investigations with other sensitives I am satisfied, for I witnessed many, and the published reports confirmed the suspicion. It is also worthy of note that other sensitives broke down or showed weakness on exactly the same points that Smith and I failed—namely, in visualizing an article difficult to describe in words signaled by a code. A regular figure or familiar object was nearly always seen by the percipient, but when a splotch of ink, or a grotesque irregular figure, had to be transferred from one brain to the other, the result was always failure. We, owing to a very ingenious diagram code, got nearer than anybody, but our limitations were great.

Smith and I, by constant practice, became so sympathetic that we frequently brought off startling hits, which were nothing but flukes. The part that fortuitous accident plays in this business can only be believed by those who have become expert in the art of watching for and seizing an opportunity. When these hits were made, the delight of the investigators caused them to throw off their caution and accept practically anything we offered. (pp. 115-7).

The reports of those trained and conscientious observers, Messrs. Myers and Gurney, contain many absolute inaccuracies. For example, in describing one of my "experiments," they say emphatically, "In no case did B. touch S., even in the slightest manner." I touched him eight times, that being the only way in which our code was then worked. (pp. 118-9).[48]

In the *Daily News* of September 4, 1911, appeared an interview with Mr. G. A. Smith from which the following quotations are taken:

"Let me say at once," he began, "that Mr. Blackburn's story is a tissue of errors from beginning to end. In the first place I most emphatically deny that I ever in any degree, in any way, when working thirty years ago with Mr. Blackburn, attempted to bamboozle Messrs. Myers, Gurney, and Podmore. Had such a thing been possible I had too much admiration and respect for them and too much respect for myself to try. These gentlemen, long before they met us, had spent years in investigating psychic phenomena, and were aware of every device and dodge for making sham phenomena. They were on the watch not only for premeditated trickery, but for unconscious trickery as well. You could not deceive them, and the quack mediums hated them in consequence." (pp. 120-1).

"He says we formed a compact to 'show up' the professors. We did no such thing. Blackburn at that time was a serious investigator, and assuredly I was. . . .

"He says I was the most ingenious conjurer he ever met outside the profession, whereas I am the worst conjurer in the world. . . . He says we had a code of signals. We had not a single one; we never contemplated the possibility of coding until we learnt it from Mr. Myers and Mr. Gurney themselves. He says we practiced together and brought off startling hits. We never did anything of the kind. He did once say what a journalistic sensation might be made by pretending the phenomena were done by trickery. . . ." (p.121).

"Do you recall, Mr. Smith, what Mr. Blackburn calls 'our great feat,' by which you, scientifically blindfolded, deafened, and muffled in two blankets, reproduced in detail an irregular figure drawn by Mr. Myers and seen only by him and Mr. Blackburn?"

"Yes, I recall it perfectly, and the discussion which followed, when Mr. Gurney said the only possible way of doing it by trickery was to conceal the drawing in a pencil case and pass it into my hand. I was amused to read two years ago in a weekly paper containing some statements by Mr. Blackburn that he gave this very explanation of how the 'trick' was done!"

"It was no trick then, Mr. Smith?"

"No, it was a bona fide experiment, and the successful result was either due to chance or telepathy. . . ."

[48] *Journal S. P. R.*, 1911-12, 15:115-9.

Mr. Smith, when questioned as to the accuracy of Mr. Blackburn's statement that he had touched him (Mr. Smith) eight times, "that being the only way our code was then worked," denied that Mr. Blackburn had ever touched him.

". . . We had no code," he said. "The whole object of the experiment was to obtain thought-transference, and all touchings were out of the question. Whenever there was any touching or contact of any description it is always minutely recorded by the observers—see the records of the Psychical Research Society.

"Further—and this is most important—none of the experiments in which Blackburn was concerned have been put forward by the Psychical Research Society in any authoritative work." (p. 122).[49]

Mr. Blackburn replied on the following day:

The fact that Mr. G. A. Smith is alive supplies another argument in support of my pet theory, that most human evidence is unreliable. I was informed of his death when I was in Africa, and since my return two persons who claimed to know him corroborated independently, while a letter I addressed to him was returned "not known." Had I been aware of his existence I should not have opened up the subject, for I am aware that Mr. Smith, as he confirms in to-day's interview, spent many of the years that have elapsed since our acquaintance in close association with leading members of the Society for Psychical Research in a fiduciary capacity. I am also aware that that position was the legitimate reward for his services in connection with our telepathic "experiments" and his undoubted power as a remarkable hypnotist.

While pleased to learn that the bright, amusing, and ingenious confrère of thirty years ago is in the prime of life, I am sorry that I should have unintentionally forced him into having to defend a position he has occupied so long. I have been reproached for postponing my confession until after the death of the principals. I am satisfied that in doing this I showed my regard for those gentlemen—Mr. Smith included—and my desire to avoid giving them pain. That Mr. Smith should have to bear the brunt of the attack is unfortunate, but quite accidental on my part.

But now to business. Mr. Smith gives a categorical denial to my story; declares that he was a genuine sensitive, and I also the possessor of psychic power. He could do no less, and I cannot blame him. He was a plucky controversialist in those younger days, and I am prepared to see him put up a tough fight now.

In most controversies there is a tendency to obscure the main issue by the introduction of minor details. Let us clear the decks of unnecessary lumber, such as the question whether I first approached the S. P. R., or they me. It is sufficient that we met. It is also agreed that Smith and I conducted many alleged telepathic experiments. It is a fact that those experiments were considered of sufficient importance then to be given first place in the official report. Mr. Smith also knows that they excited great interest, and that he and I were made much of by many men of scientific eminence. To attempt to belittle the importance of those experiments now is childish. No doubt greater things have been brought off since, but we were the pioneers, and I am satisfied that we unintentionally gave scores of subsequent experimenters the cue how to become "telepathic sensitives."

[49] *Ibid.*, pp. 120-2.

Mr. Smith denies that we employed a code. My reply is that without one it would have been impossible for me to convey to him the figures drawn by the members of the committee for transference from my brain to that of the blindfolded, blanket-muffled, sensitive Smith. Let us dismiss all the other successful experiments—any one of which I will undertake to repeat to-day under identical conditions, with the aid of any intelligent confederate—and confine ourselves to "our great feat," which Mr. Smith tells your interviewer he recalls perfectly. That feat, if genuine, would establish telepathy beyond cavil. All others sink into insignificance in comparison. It was a master stroke, and so great was the impression produced by it, both upon the "best trained and best qualified observers in London" and ourselves, that we decided to retire upon our laurels, feeling certain we could never hope to repeat or equal it. It was the best and last thing I did. As Mr. Smith repudiates participation in the invention, I will take full credit or otherwise for it. I ask that readers will note very carefully every detail in the ensuing description of the trick, for it is they who will have to give the verdict.

The committee had realized the possibility of conveying by signals a description of a regular figure or any object capable of being described in words, and I would direct the attention of those who have access to the printed copies of the early figures Smith and I produced. It will be noticed that so long as the figures were describable in words they were fairly accurate reproductions; but the more irregular and indescribable they became the greater and wider the discrepancies between the original seen by me and the copy produced by Smith. Now I put it as a fair question: If Smith could see what I saw, as he professed, why is it that he could see plainly an equilateral triangle, but fail to see it if one of the sides or angles was "wobbly" and out of shape? Again, if he could reproduce with reasonable accuracy the silhouette of a man's head, easily described by a code, why did he fail when that same head was touched up with black ink protuberances, with the nose under the chin, a big ear on the back of the head, and so on? The reason was simple. Our code was confined to regular, or fairly regular, figures. It would have taken hours to spell out a full description of that figure by the sounds, movements, intervals of time, bogus mesmeric passes that stirred his hair, and the numerous, almost imperceptible, signals that formed perhaps the most complex and effective code ever used by conjurers. I doubt whether any person could write at leisure a description of such an object so accurately as to enable another one to reproduce the figure from that description.

This reasonable point of view occurred to the committee, and they abandoned regular figures for complex indescribables. Need I say that we failed again and again? In fact, we ceased any attempt to "transfer" them. I had a signal, which I gave Smith when the drawing was impossible. We made a pretense of trying hard, but, after a time, would give up on the stock explanation of "absence of rapport." Mr. Smith is angry with me for holding in light esteem the capacity of Messrs. Myers and Gurney for taking precautions against deception. I confess that their irregular drawings completely snuffed out the psychic power which, according to Mr. Smith, I possessed without knowing it. As a matter of fact, the committee were beginning to have grave doubts when the "great feat" I shall now describe saved our reputations and enabled me at least to carry out my bat.

These were the conditions: Smith sat in a chair at a large table. His eyes were padded with wool, and, I think, a pair of folded kid gloves, and bandaged

with a thick dark cloth. His ears were filled with one layer of cotton-wool, then pellets of putty. His entire body and the chair on which he sat were enveloped in two very heavy blankets. I remember, when he emerged triumphant, he was wet with perspiration, and the paper on which he had successfully drawn the figure was so moist that it broke during the examination by the delighted observers. Beneath his feet and surrounding his chair were thick, soft rugs, rightly intended to deaden and prevent signals by feet-shuffles—a wise precaution, for in our early experiments my feet did marvelous things. Smith being rendered contact proof and perfectly insulated, my part began.

At the farther side of the room—a very large dining-room—Mr. Myers showed me, with every precaution, the drawing that I was to transmit to the brain beneath the blankets. It was a triangle of heavy black lines, interlaced, some curved, some straight, the sort of thing an infant playing with a pen or pencil might produce, and I am certain absolutely indescribable in words, let alone a code. I took it, fixed my gaze on it, pacing the room meanwhile and going through the usual process of impressing the figure upon my retina and brain, but always keeping out of touching distance with Smith. These preliminaries occupied perhaps ten or more minutes, for we made a point of never hurrying. I drew and redrew the figure many times openly in the presence of the observers, in order, as I explained and they allowed, to fix it on my brain. I also drew it, secretly, on a cigarette paper. By this time I was fairly expert at palming, and had no difficulty, while pacing the room collecting "rapport," in transferring the cigarette paper to the tube of the brass protector on the pencil I was using. I conveyed to Smith the agreed signal that I was ready by stumbling against the edge of the thick rug near his chair.

Next instant he exclaimed: "I have it," his right hand came from beneath the blanket, and he fumbled about the table, saying, according to arrangement: "Where's my pencil?"

Immediately I placed mine on the table. He took it and a long and anxious pause ensued.

This is what was going on under the blanket. Smith had concealed up his waistcoat one of those luminous painted slates which in the dense darkness gave sufficient light to show the figure when the almost transparent cigarette paper was laid flat on the slate. He pushed up the bandage from one eye, and copied the figure with extraordinary accuracy. It occupied over five minutes. During the time I was sitting exhausted with the mental effort quite ten feet away.

Presently Smith threw back the blanket and excitedly pushing back the eye bandage produced the drawing, which was done on a piece of notepaper, and very nearly on the same scale as the original. It was a splendid copy. (pp. 123-6).[50]

In a second interview published in the *Daily News* on September 6th Mr. Smith said:

It is the most amazing piece of invention ever brought to my notice. . . . All the essential points of Mr. Blackburn's article are untrue, and I deny the whole story from beginning to end. (p. 127).[51]

[50] *Ibid.*

[51] *Ibid.*

Members of the Society came forward with expressions of confidence in Mr. Smith:

Professor Barrett said:

> Now I was present at that experiment, and you may say that not only I, but Myers and Gurney, had the most absolute confidence in Mr. Smith. (p. 129).[52]

Mrs. Henry Sidgwick said:

> . . . All communication of the leading workers in psychical research with Mr. Blackburn ceased not long after the experiments in question, and, . . . on the other hand, the connection of Mr. G. A. Smith with the work of the society was long and intimate. (p. 130).[53]

Mr. Blackburn's statement is confirmed to a certain extent by an article in the *Westminster Gazette* for January 29th, 1908. There Sir James Crichton-Browne describes how he and Dr. Francis Galton on one occasion were present, as outsiders invited by Professor Romanes on behalf of some members of the S. P. R., at the Dean's Yard experiments with Smith and Blackburn. He tells us that Smith, the percipient, was blindfolded and succeeded in reproducing a few regular or simple figures, such as an owl, the diagram on which Blackburn had concentrated his mind in one of the experiments. But Smith completely failed in reproducing an irregular figure which Professor Romanes and Sir James Crichton-Browne subsequently insisted on as a test when they began to suspect the use of a code. The next diagram was the shield on Dr. Galton's signet ring. This had an oval shape, but the reproduction was of a triangular shield. Sir James Crichton-Browne continues: "By this time I was quite satisfied that Mr. S. was not effectually blindfolded, and that it was practicable for Mr. B. to communicate with him both by sight and hearing; so Romanes and I asked permission, which was granted, to blindfold him anew. We proceeded to do so *secundum artem*. Cotton-wool was procured, the sockets were packed, the ears were plugged, and a large handkerchief made all secure. After that several experiments were tried as before, but there never was the smallest response on the part of Mr. S. to Mr. B.'s volitional endeavors. There was no more flashing of images into his mind. His pencil was idle. Thought-transference was somehow interrupted." And he concluded the account as follows: "I was invited to be critical and sceptical, and I was so. I daresay more credulously inclined people will think that my suspicions were unjust and that no trick was practised—that was clearly the feeling of some of the psychical researchers present. The last scene of all, or passage-at-arms, I vividly recollect. Mr. Myers, standing in front of the fireplace, said, 'It must be allowed that this demonstration has been a total failure, and I attribute that to the offensive incredulity of Dr. Crichton-Browne.' To which I rejoined 'I hope I always will show offensive incredulity when I find myself in the presence of patent imposture.'"[54]

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[52] *Ibid.*

[53] *Ibid.*

[54] Tuckett: Psychical researchers and "the will to believe." *Bedrock*, 1: 197-8.

In the Blackburn-Smith controversy misstatements were made on both sides; but the most serious of these were made by Smith and his colleagues in attempting to show that the experiments concerned had not been regarded by the Society as important, whilst the reader in access to the *Proceedings* (1882-3) may see for himself that they were portrayed by the Society in its most authoritative publication as evidence of the first rank. This misrepresentation certainly discredits the Society's case. It is indeed clear that at some time between the publication of the Committee's Third Report (April 24, 1883), and the publication of the "Phantasms of the Living" (June 1886), the Society determined to suppress the re-publication of this series of experiments: they were not re-published in the "Phantasms of the Living"; they are not included in Podmore's or Thomas' resumés; even Barrett omits them in his book on "Psychical Research" which appeared in 1911.[55]

Why?

Since Smith was a percipient in these experiments in which the reproduced diagrams show unmistakably and decisively in every published case a relation to the originals which cannot be attributed to chance, does not the Society in suppressing the experiments impeach Smith as a reliable party to a thought-transference experiment? This situation is particularly unfortunate, for the reason that in all of the later principal series of officially conducted experiments in thought-transference which the Society regards as the most successful the hypnotist and agent was Mr. Smith.

But apart from the controversy and the inexplicable position of the Society, what can the inquiring reader make out of this series of experiments? Are the undoubted similarities between reproductions and originals to be regarded as evidence of thought-transference or of code?

1. The similarities between reproductions and originals in both the geometric and the fantastic figures, are evident in gross outline of the whole drawing or of its separate parts the relations of which are not preserved, and are just such similarities as one should expect from the use of movement or verbal code (a flagrant and decisive instance is No. 7, *Proceedings,* 1:185). The detail which would be transferred with a visual image is absolutely lacking; it was the idea, not the design, that

[55] It was probably an oversight that the Society permitted these experiments to be mentioned and some of the drawings to be reproduced in "The Society for Psychical Research, its rise and progress and a sketch of its work," which was published in 1903 by Edward T. Bennett, for twenty years Assistant Secretary of the Society.

was communicated. This deviation in similarity is far beyond what may be attributed to Mr. Blackburn's fading or changing imagery of the figure.

2. In the guessing of colors and of names, the increased approximation in successive trials is a characteristic of code reading when the code is working under difficult conditions.

3. The errors in guessing numbers are precisely code errors: for 35, 34; for 48, 58 (Experiments 9 and 10).[56] It is this type of error that occurred so frequently in the guesses of the Creery sisters[57] and also in the responses of "Clever Hans," the "educated" horse of Herr Von Osten.[58]

Internal evidence points decisively to the use of code in this series of experiments. This hypothesis is congruent with the external evidence:

(1) Smith had been engaged with Blackburn in giving exhibitions of thought-reading in Brighton prior to these experiments, and no public exhibitions of thought-reading, thought-transference, or telepathy of any sort, are ever given without the use of code. He was accustomed therefore to reading code signaled by Blackburn. The probability that he discontinued the use of code with the entrance of Gurney and Myers among the spectators, is not very crushing.

(2) The sudden permanent interruption of the investigation was not expected by the Committee, since further experiments were in view,[59] and was not welcomed by them for the reason that they hoped to get so much further decisive evidence as would be necessary to break down the skeptic's hypothesis of code as completely as their own doubts had been removed by the reproductions in Fig. 22. This unexpected interruption, just at a most interesting point in the investigation, would be entirely consistent with the motives[60] of two young adventurers who had in a spirit of mischief carried their enterprise to a point where they were both conscience-stricken with the gravity of their perfidy, or fearful of serious disaster through exposure.

(3) The later repudiation of the investigation by the Society implicates Smith in code-reading.

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[56] *Supra*, p. 478.

[57] *Supra*, pp. 466, 475-6.

[58] Pfungst: Clever Hans. New York, 1911, p. 144.

[59] *Vide, Proceedings S. P. R.*, 1: 165.

[60] Suggested in the fourth paragraph of Mr. Blackburn's first letter, *supra*, p. 484.

If this is the status of the best of the officially conducted experiments with normal percipients, what must be thought of all the rest of the Society's evidence for thought-transference? Simply because Smith and the Creery sisters were the only parties to the experiments who have been known to use code is no guarantee that other experimenters not originally under the control of the Committee were free from either code or other deceptive devices. The third series of experiments upon which the Society placed reliance for proof of thought-transference to a normal percipient was conducted by Messrs. Malcolm Guthrie and James Birchall, in Liverpool. Both investigators were inexperienced in this sort of investigation. The young ladies, who were employed in Mr. Guthrie's drapery establishment, and with whom the experiments were made, began their practice of thought-reading in imitation of Irving Bishop, and although the reports of this series of experiments occupy considerable space in the Society's *Proceedings*,[61] the published results give internal evidence of code.[62] Concerning this series, Moll writes:

> There was a time when some of the telepathic experiments carried out in England—more especially those made by Guthrie and Birchall—appeared to me, relatively speaking, free from error. Nevertheless, when I take into consideration the way in which the reports are drawn up, I am compelled to admit that those experiments are not convincing.[63]

G. Stanley Hall wrote:

> There is little indication that Mr. Guthrie is aware of the number and subtilty of the sources of error in such experiments as he conducts.[64]

The best evidence the Society has for thought-transference to an hypnotized percipient lies in the results of two series of experiments [65] in which Mr. G. A. Smith was both hypnotist and agent.

Podmore was probably right in his statement quoted above,[66] to the effect that the *crux* of the Society's proof of thought-transference lies in the possibility that information was "unconsciously conveyed by normal channels from agent to percipient," in that this *vera causa* may reasonably be conceived to have operated in all cases where collusion is not

[61] 1882-83, 1: 263-283; 1883-84, 2: 24-42, 189-200; 1885, 3: 424-452.

[62] The peculiar code-error of missing a number or letter just one or two places is found in almost every error on numbers or letters.

[63] Moll: Hypnotism. 4th ed., p. 552.

[64] *American Journal of Psychology*, 1: 137.

[65] *Proceedings S. P. R.*, 1889-90, 6: 128-170; 1892, 8: 536-596.

[66] *Supra*, pp. 483-4.

a sufficient explanation. Unconscious indications of this sort would have put the Creery sisters who were sometimes excluded from "agency" into a position to make effective use of their code.

That the Society has not been alive to the difficulties of investigation in this field is evident in its publications from the beginning to the present time. Circular No. 1, published in December, 1883, under (1) Thought-transference, says:

> What is needed, then, is a large number of experiments similar to those already published, but conducted by different groups of persons and under different conditions. We have reason to believe that the percipient faculty, so far from being abnormal or infrequent is pretty generally diffused; and if this fact is gradually made good among persons of recognized position and intelligence, attempts to explain the results by trickery and collusion will become increasingly ridiculous. Only thus, moreover, can we obtain sufficient material upon which to base generalizations.[67]

Similar appeals have been made for general and incompetent assistance to within recent years.[68]

The general reader can get a fair idea of the difficulty of such investigation from two fairly popular and readable accounts of what may be regarded as elementary investigations, the first being much more rigorous than the second in procedure and extent of work: (1) The report of a German psychologist, Dr. Pfungst, on the processes employed by the celebrated horse "Clever Hans," in his apparently intelligent responses to questions;[69] and (2) the report of Professor Münsterberg on his investigation of the little telepathist, Beulah Miller, of Rhode Island.[70] Both of these investigations were stopped short with preliminary yet significant findings. Serious investigation of thought-transference would, of course, be pressed to a conclusion with respect to a complete description and classification of the various laws found to govern the phenomena. Certainly no drawing-room experiments can command the respect of scientists.

Largely because of the Society's failure to realize the difficulty of thought-transference investigation, and of its fatal overestimation of the capacity of astute observers to cope with collusionists (always, of course,

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[67] *Proceedings S. P. R.*, 1893, 1: 297.

[68] *Journal S. P. R.*, October 1911, 15: 131-2.

[69] Oskar Pfungst: Clever Hans, A Contribution to Experimental Animal and Human Psychology. Tr. by Rahn. New York: Holt, 1911.

[70] Münsterberg: Psychology and Social Sanity. New York: Doubleday, 1914, pp. 141-177.

in a limited series of trials of which the collusionists retain complete control), its policy in amassing evidence has embraced a serious fallacy —the "fagot theory":

> It is not necessary . . . that any individual's honesty shall be completely assumed, in the sense of being used as a certain basis for conclusions. The proof must depend on the number of persons, reputed honest and intelligent, to whom dishonesty or imbecility must be attributed if the conclusions are wrong, *i. e.*, it must be a cumulative proof . . . enough sticks must be collected and tied together to make a fagot of a strength which shall defy suspicion.[71]

The keenest scientific observer may fail utterly to detect a skillfully worked code, without meriting the charge of either imbecility or carelessness, and he may be an unconscious party to sensory communication, without being dishonest.[72] Untrustworthiness, in this sense, may be extended indefinitely[73] without conferring upon observers any moral obloquy. It is only after they have had the opportunity to learn from professional criticism their disabilities, owing to their lack of "technical knowledge and skill"[74] in this field, that they are really responsible for the unsoundness of their conclusions. An aggregate of scientifically untrustworthy experimental evidence can never become a fagot of proof,—the signs are all minus.

We have recently had a good illustration of the fact that the spread of responsibility is no guarantee of a sound conclusion, even when the responsibility is shared by scientific men more or less eminent in the particular field of knowledge to which the observed phenomena belong. And since this episode in the history of physical science also shows how potent unrecognized psychological causes are, even in the most objective and tangible field of science, we cannot resist the opportunity to scan its lesson.

The "N"-Ray Delusion.

If one consults the various volumes of *Science Abstracts, Section A. —Physics,*[75] and looks through the indexes under "Rays and Radiations" for "*n*-rays," he finds a curious fact: Before 1904 and since 1909 no abstracts on the topic appear; but beginning with 1904, for each successive

[71] Phantasms of the Living, pp. 19, 20.

[72] *Cf.*, Quotation from Gurney, p. 465, *supra.*

[73] *Cf.*, Quotation from Stewart, pp. 464-5, *supra.*

[74] *Cf.*, Quotation from Sidgwick, p. 468, *supra.*

[75] Published in London and prepared by the Physical Society of London, with the assistance of internationally affiliated organizations.

year, their number is as follows: 77, 8, 3, 1, 0, 1. A closer inspection of the literature reveals the following remarkable history:

Professor Blondlot, the eminent physicist of Nancy, who was elected a correspondent of the Academy of Science to fill the vacancy caused by the death of Helmholtz, discovered the *n*-ray (so named in honor of Nancy).[76] The presence of the *n*-ray was determined by the decrease of the resistance of a spark-gap, by the increased glow of a platinum wire, by the increased luminosity of a phosphorescent surface.[77] It was found to emanate from the solar radiation and from bodies in which it was stored.

The discovery was promptly confirmed by other scientists: C. Gutton, Mascart, Virtz, H. Bordier, E. Bichat, J. Meyer, J. Becquerel, J. M. de Lépinay, H. Bagard, E. Rothé, G. Sagnac, and others. As investigation progressed *n*-ray knowledge became more refined and hypotheses more complex; under some circumstances the phosphorescent screen was dimmed when it was expected to become more luminous, and n_1-rays with antagonistic or contrary effects were discovered. Breydel discussed n_2-rays.[78] Ponderable rays or "heavy emissions" were discovered.

Applications of the new knowledge were made in fields outside of physics. Corson applied *n*-rays to chemistry;[79] M. Lambert and E. Meyer studied their effects upon biological phenomena;[80] E. Meyer upon plants;[81] Bohn applied them to explain irregular tropic reactions of a nereid; A. Charpentier found that compression of a nerve, and that activity in nerve-centers and in certain portions of the brain, were accompanied by the emission of *n*-rays;[82] and that the application of the *n*-ray to the seventh cervical was accompanied by the dilation of the pupil varying from 0.5 mm. to 1 mm.;[83] that its application to the sense-organs or the proper nerve-centers lowered the threshold of sensitivity;[84] that the maximum effect as shown by the screen was obtained by application of the *n*-ray to the Rolandic region of the skull;[85] G. Ballet observed the emission of

[76] *Vide*, Blondlot: "N" Rays. Tr. by Garcin. London, 1905.

[77] The determinations had to be made by the eye since the *n*-ray gave no photographic effect.

[78] *Electr.* Paris, 1904, 41: 325.

[79] *Comptes Rendus des Séances de l'Académie des Sciènces*, 1904, 138: 1423-5.

[80] *Ibid.*, pp. 1284-5.

[81] *Ibid.*, pp. 101-2, 272-3.

[82] *Ibid.*, 1903, 137: 1277-80.

[83] *Ibid.*, 1904, 138: 270-2.

[84] *Ibid.*, pp. 584-6, 648. This fact explained to Blondlot why the change in luminosity affected the eye and not the photographic plate.

[85] *Ibid.*, pp. 715-7.

n-rays from the pathological portions of the nervous system;[86] A. Broca studied the relation between *n*-rays and the brain;[87] A. Broca and A. Zimmèrn studied the condition of the spinal cord as shown by *n*-ray emission;[88] and it appears that M. di Brazza, a student at Liege, discovered rays emitted from an active brain which differing from the *n*-ray he called *i*-rays (in honor of Italy).[89]

In spite of this generous "spread of responsibility" the literature contributed adverse criticism; other physicists carefully repeated Blondlot's experiments and found no *n*-rays: Donath at Berlin, Kaufmann at Bonn, Clossen at Hamburg, Pacini at Rome, and Zahn, O. Lummer, C. C. Schenck, E. Gehrcke, W. A. Nagel, Rudge, McKendrick and Colquhoun, F. Bonola, G. Cavina, A. Turpain, E. Salvioni, A. A. C. Swinton, among others. The situation was precarious. The *n*-rays could not be found outside of France, and the discussion threatened to take on the color of a national issue. In the latter part of 1904 (November 5 to December 10) the *Revue Scientifique* made a census of opinion revealing a substantial number of French scientists among the opposition.

The potency of unrecognized psychological causes for error is shown by the head-long course of the *n*-ray delusion in the face of current criticism that pointed out the error specifically. As early as January 21, 1904, A. A. C. Swinton, who had repeated most of Blondlot's experiments without finding a trace of the phenomena described by Blondlot, suggested [90] that the observations must be due not to physical but to physiological processes, which are not operative in the case of all persons; and on June 4, 1904, E. Salvioni, who having had poor success in reproducing Blondlot's phenomena had turned to studying the *subjective* sources of error, announced [91] his conclusion that a particular condition of sensibility is required; that the phenomena are comparable to the luminous phenomena of the so-called animal magnetism, and odic force. A. Turpain made a number of observations in order to detect the action of *n*-rays upon a phosphorescent screen, using all the supposed sources of *n*-rays, such as a Nernst lamp, a file, and a magnetic or Hertzian field, and the affirmative results amounted to 80%; but on arranging the experiments in such a manner that the observer did not know

[86] *Ibid.*, pp. 524-6.
[87] *Ibid.*, pp. 1161-3.
[88] *Ibid.*, pp. 1239-41.
[89] *Harper's Weekly*, 1905, 49: 69.
[90] *Nature*, 69: 272.
[91] *Atti della R. accademia dei lincei.* Roma, 13: 610-6, 703-6.

whether the action of *n*-rays was to be expected or not, he found that the affirmative results were reduced to 50%, thus showing that self-suggestion plays the most decisive part in them.[92] And R. W. Wood, of Johns Hopkins University, when in France, visited a laboratory to witness the phenomena, and found that when the observer did not know what to expect his observations were in error.[93]

Physicists are agreed that the *n*-ray is a delusion.[94] All of its phenomena have been assimilated to known causes. But a delusion, once supported by men of eminence, is well-nigh immortal; it will find its adherents in men eminent in other fields and in the intelligent but non-technical public; its applications ever grow in variety. It is likely that *n*-rays will still be used to measure the wave length of oscillations in the nerve of a frog; to locate and diagnose pathological conditions of the body; to find and identify overtones; to determine the psychic power of a person; to analyze his character;[95] to explain telepathy,—for are not the emissions of *n*-rays from the brain the long suspected "brain-waves"? At any rate "brain-waves" have been often suggested[96] as explanatory of telepathy, the transmission of thought by radiation has been studied,[97] and, finally, *n*-rays have been employed to explain telepathy.[98] Tunzelmann, five years after the hypothesis had been officially buried, accounted for the transference of images from his family circle to his own mind, in a few thought-transference experiments he was inspired to try after reading Hudson's "Law of Psychic Phenomena," by the *n*-rays traversing the closed eyelids and the skull (p. 628).[99]

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[92] *Bulletin des Séances à la Société Française de Physique,* 1906, 1:94-100.

[93] *Vide, Harper's Weekly,* 1905, 49:57-8.

[94] *Vide,* G. F. Stradling: A resumé of the literature of the *n*-rays, the n_1-rays, the physiological rays, and the heavy emission. *Journal of the Franklin Institute,* Philadelphia, 1907, 164:57-74, 113-130, 177-199. Reference is made to 278 articles.

[95] Hooker says the emitted rays from a passionate man have a red hue; from a well-intentioned man, pink; *cf., Lancet,* London, October 1904, 2:1380.

[96] J. T. K.: *Spectator,* January 30, 1869, pp. 136-7; *cf.,* also, *ibid.,* 133-4; Barrett, Gurney, and Myers: Phantasms of the Living, vol. I, pp. 111-2; *Nineteenth Century,* 1882, 11:900; Mark Twain: *Harper's Monthly,* December 1891, 84:100 ff.; Crookes: *Proceedings S. P. R.,* 1896-97; 12:352 ff.; Podmore: Naturalization of the Supernatural, 1908, p. 11; Barrett: Psychical Research, 1911, pp. 107 f.

[97] Benigni: Observation sur la propagation de la pensée par radiation.

[98] Kéraval: Rayons *n* et transmission de la pensée, *Echo Med.,* 1904, p. 51; Tunzelmann: A Treatise on Electrical Theory. London, 1910. Appendix R, pp. 626-632.

[99] *Op. cit.*

The chief lessons to be drawn from this brief narrative are:

(1) That "spread of responsibility" in establishing a *matter of fact,* even in a field where the facts are regularly identified by wholly objective criteria, and even when the experimenters are men of eminence in the field to which the observed phenomena properly belong, is no guarantee of the existence of the alleged fact, unless the responsibility is shared by the main body of authorities in that field; the "fagot theory" is fallacious.

(2) That unrecognized psychological sources of error are potent for delusion, even when the investigators are familiar not only with the experimental method, but also with the field of phenomena in which they are working, and even when the sources of error are specifically pointed out by capable critics.

(3) And that since the investigators may be unable to profit from the critics, it would seem that, where psychological sources of error are concerned, some experience in experimental psychology is necessary in order to understand precisely what the endangering psychological processes are and how to reckon with them in the control of experimental conditions.

Conclusion.

Recurring to the general problem of the trustworthiness of the existent evidence for telepathy, the reader has probably found in this appendix sufficient grounds for scientific caution.

The best of the evidence, that which won over the agnostic members of the Committee of the S. P. R. to the belief in thought-transference to a normal percipient, and which occupied first place in the Society's authoritative *Proceedings,* has suffered a fatal decline. The Committee showed caution in guarding against fraud and error and expressed their belief that both had been eliminated. Contemporaneous critics pointed out the possible operation of both, and challenged the trustworthiness of the results. Collusion was later confessed by one or more of the parties to the experiments in both of the principal series. As against the presumption of false confession, or a segregation of untainted results, there remains in the published records of both series of experiments internal evidence of the use of code. Within three years of the conclusion of both series, the Society apparently sought to suppress their re-publication, although it has permitted recent resumés of both. The Committee fully expected to have those most successful results freely verified in further and more stringently controlled experiments with the same and

with other favorable percipients; and also to find in the course of investigation a general distribution of the telepathic capacity among normal persons. Their own percipients suddenly declined to continue the experiments, supporting the hypothesis of collusion, the successful results have never been equaled with other percipients, and the imposing mass of experimental results published from other centers of investigation, not to mention a very considerable amount of negative results which has come to the notice of the Society and which it has declined to publish,[100] controverts the expectation of the Committee that the telepathic capacity is common.

The best of the Society's evidence for thought-transference to a hypnotized percipient was obtained under unsatisfactory conditions: Instead of employing as hypnotist and agent a scientific man, such as Dr. Bramwell, who was also one of its members, the Society employed a young man who had previously been engaged in giving public demonstrations of hypnotism and of thought-reading, and who was a party to one of the series of experiments which was later suppressed by the Society and which, according to the internal evidence of the published reports, depended upon collusion in the use of code for its successful results.

Apart from the unsatisfactory nature of the results of the experiments, the general conduct of the investigation casts discredit upon the conclusions drawn from the data. Appeal was constantly made for the assistance of incompetent investigators, which, besides facilitating the accumulation of results which must be entirely untrustworthy through gross error in procedure and records, encouraged skilled collusionists to match wits with the Society's representatives in a contest so unequal that positive results of a spurious character were insured. The "fagot theory" which provided for a "proof" of telepathy by the "spread of responsibility" over so large a number of reputable persons in the control of the various series of successful experiments that it would be easier to believe that telepathy is a fact than to believe that all of the reputable persons were in error, is fallacious, in that the difficulties involved in such investigation were not duly appreciated.

The "spread of responsibility" for the establishment of the psychological phenomenon of telepathy is much inferior to the "spread of responsibility" for the establishment of the physical phenomenon of the *n*-ray, since many men eminent in the field of physics were involved in

[100] *Vide,* quotation from Thomas, p. 26, *supra.*

the latter, and but very few eminent experimental psychologists are involved in the former, yet the body of competent physicists agree that the *n*-ray was a delusion. Unless the "spread of responsibility" for telepathy can be extended to include the main body of experimental psychologists, telepathy will certainly share the fate of the *n*-ray, and the textbooks in the history of psychology will speak of the "thought-transference delusion." In the one case, *n*-rays could be found only by physicists, in physical laboratories, in France in 1904-6; in the other, thought-transference could be found only by eminent men who were not psychologists, in places other than psychological laboratories, in England and Ireland in 1882-92.

The reader should not regard the body of psychologists as standing firmly upon the laws of the "isolated conductivity" of the neurons, the "centripetal" course of nervous impulse in hallucination, and the inviolability of the sensorial gateway, and dogmatically decreeing telepathy out of existence; the psychologists recognize the possibilities of advancement of knowledge in their field and they would survive telepathy, should it appear with the proper credentials. They would even welcome it with open arms, just as physicists have welcomed α, β, and γ rays. Their agnosticism is scientific, not dogmatic.

Neither should the reader get the impression that the Society for Psychical Research has not justified its existence, for it has been of great service to mankind, particularly in helping it to rid itself of fraud and delusion, and in stimulating neglected lines of research from which great benefits are sure to come. No final or decisive knowledge concerning telepathy would have been likely for centuries had it not stimulated direct research in it. Even the errors in its investigations are of value in compelling a more accurate definition of the scientific method, and in illustrating our utter dependence upon this method for sound knowledge of all matters pertaining to the phenomenal world.

The errors made in the conduct of the investigation of thought-transference bear to present and future investigators a valuable lesson for which credit is due the eminent members of the Committee: *technical experience in experimental psychology is requisite both for the control of the experiments and for the interpretation of the results.* It is to their credit that the questionable evidence was suppressed, and it is regrettable that any members of the Society promote the telepathic hypothesis while it occupies so questionable a status. Unfortunately, so long as there is so great a disparity in the capacities of the various students of the problem of telepathy to properly evaluate the evidence in

the experimental results, so long may the interested and inquiring public expect to be presented with opinions as varying and contradictory as are the following:

Professor W. F. Barrett, in 1911:

> Thought-transference . . . is not, in my judgment, a subject of controversy. It is established beyond the possibility of challenge to those who will really examine the evidence.[101]

Ivor Ll. Tuckett, in 1912:

> In spite of Professor Barrett's *dictum,* the fact remains that several of the more critical psychical researchers who have thoroughly examined the evidence have ultimately come to the conclusion that the case for telepathy is completely unproven.[102]

Professor C. S. Minot, in 1895:

> After a thorough examination of the evidence adduced, I am brought to the conclusion that thought-transference, even as a hypothetical explanation, is a superfluous conception.[103]

Professor G. Stanley Hall, in 1896:

> We are profoundly convinced [that] the entire telepathic presumption is yet very far from being a *prima facie* case, is premature at best, and that it is at present with its rank mazes of mystic guess-work a source of befuddlement and obfuscation.[104]

Accepting Professor Sidgwick's *dictum* that the agreement of experts must be the final test,[105] the statement of Gurney that the standard of evidence must be drawn from the recognized departments of knowledge,[106] together with Professor Sidgwick's definition of *sufficient evidence,*[107] it is certainly true that the Society, on account of its fiascoes and its persistent lack of psychological vision, is immeasurably farther from its goal today than it was in 1886, in its effort to produce *proof* of thought-transference.

Hope for the Society lies in adopting the program of the Hermit of Prague[108] and in coöperating with a psychological laboratory.

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[101] London *Daily News,* September 6, 1911; quoted in *Bedrock,* 1: 180-1.

[102] *Bedrock,* 1: 193.

[103] *North American Review,* 160: 227.

[104] *American Journal of Psychology,* 7: 137. For other quotations from students of the experimental data, *vide,* "The Present Status of Telepathy," in Part I, pp. 17 ff., *supra.*

[105] *Supra,* p. 468.

[106] *Supra,* p. 462.

[107] *Supra,* p. 463.

[108] *Supra,* p. 28.

APPENDIX D.

INVESTIGATION WITH A "TRUMPET" MEDIUM.

In the Autumn of 1913 the writer (The Fellow in Psychical Research) became a member of the California Psychical Research Society, the object of which is, as stated in its Constitution, "to carry on and to promote investigation of psychical and supernormal phenomena," and "to develop a scientific interpretation of them." Owing to the compatibility of the aim of the Society with the purposes of the Division of Psychical Research, a program of coöperative research was immediately entered upon. After a season's work, in accordance with a provision of the Constitution that "The Reports of the investigation committee shall be furnished the members," the following Report was drawn up, read to the Society, accepted, and printed. As internal evidence will show, its main purpose was (*a*) to illustrate a scientific method of investigation, (*b*) to make specific, through its preliminary findings, certain critical problems for research, and (*c*) to propose a program for continued investigation which should be of scientific value. Although the Report was addressed to the Society and its form was largely determined by the need of arousing the members to a consciousness of the importance of scientific work, its method and program were thought to be of sufficient interest to students of psychical research to warrant its permanent record in professional publications.

The writer acted as experimenter for the investigation committee, and assisted in drawing up the report.

INVESTIGATION WITH A "TRUMPET" MEDIUM.[1]

By the California Psychical Research Society.

Mrs. Key,[2] who has given series of séances before psychical societies and prominent investigators in Boston, Paris, and California,

[1] This report is reprinted in a slightly revised form from the *Proceedings of the American Society for Psychical Research,* New York, 1914, 8: 201-252. Obligations to the American Society are hereby acknowledged.

[2] The identity of the psychic the Society withholds for the reason that her reputation as a semi-professional medium might suffer temporary injury through the experimenter's part of this preliminary report. References in the literature of psychical research, the names of organizations and investigators, the places of investigation, and the names of the psychic's customary controls, consequently, cannot be specifically given in this report.

placed herself without pay at the service of the California Psychical Research Society for investigation of the phenomena which occur in her presence.

She is an American gentlewoman of refinement and culture, and is known to her closest friends for her high moral character, and to the members of the Society for her sincere and whole-hearted coöperation in investigation.

That she is to be classed as one of the more remarkable psychics of this generation is attested by the various reports upon the investigation of her phenomena, and by members of our Society.

A noted student of psychical research, after several sittings with her, brought her to the attention of investigating bodies. He published in 1892 a report in which he stated that she was entirely disconnected in a physical way from the phenomena which occurred about her; and that she not only cheerfully submitted to crucial conditions but welcomed "tests." In the subsequent investigation, involving many sittings, the report of which was published in the following year, objects several feet beyond her reach, according to measurement, were moved, handled, and deftly manipulated in touching the sitters in the dark; writing occurred on books and sheets of paper; raps were intelligently given, and "trumpet" voices spoke. Before the phenomena began, the psychic had been bound and the bindings sealed to her chair, and after the séance was over, all bindings were found to be intact. One of the eminent witnesses saw the hand of the control, which had just written upon a book, pass, upon request, rapidly up and down, with fingers outspread, between himself and a streak of light, six feet from the psychic's left hand.

A noted doctor, upon another occasion, in 1907, recorded in a reputable journal his astonishment at the precision of the movement of the trumpet in the dark, while the psychic was securely bound to her chair.

An investigator of international reputation attests to the power of prevision on the part of her controls. He had arrangements made for a journey, with a ticket in his pocket determining a definite route. He was told in the séance communications that he would make the journey, but that he would not take that route and that many things of a most unusual and unexpected nature were to occur. All these events, not in the least capable of being influenced by any state of his mind aroused by the astonishing prophecies, transpired.

Some of the members of our Society who have had sittings with the more renowned psychics at home and abroad attest that both the psychical and the physical phenomena which occur in this psychic's presence are equal to any they have ever witnessed.

The Society is confident, therefore, that the phenomena it is investigating are of the highest order, and that its findings will in no sense descend to the common exposé of conscious fraud, but will make a substantial contribution to knowledge in this field.

Investigation began in September, 1913, (after November 1st, in the rooms of the Society, at 320 Market street, San Francisco) and continued until May 9, 1914. The séances were held Saturday afternoons, usually from 2 to 5 o'clock, in complete darkness. The number of sitters varied from 3 to 12, but usually numbered about 8.

Owing to the alleged difficulty of keeping "test" conditions for the customary phenomena, the Society resolved to use methods of investigation similar to those employed abroad with Eusapia Palladino,[3] and in March Dr. Coover, with whom the Society has been coöperating throughout the year in investigating clairvoyant and telepathic capacity in psychics,[4] consented to undertake the experimenter's task of providing and controlling such scientific instruments as are calculated to portray accurately and objectively what takes place. His report follows:

DR. COOVER'S REPORT.

The writer feels a bit apologetic in reporting upon the investigation of "physical phenomena," not only because its history is unsavory, but because he had resolutely set himself to the task of determining whether "psychics" possess clairvoyant or telepathic powers, and of subjecting whatever processes should come to hand to psychological analysis and study. His apparatus consisted of a pack of playing cards and a dice-box, and he was inclined to take the "voices" for what they proclaimed themselves to be, so long as they would name his cards.

But psychological research came to a halt, in the Society's séances, in an unexpected manner. The "independent" voices, who could report upon the safety of relatives in Mexico and could define the attitude of Japan toward the quarreling republics, could not, even after months of effort, bring themselves to naming the cards. They could see the card and they had the power of speech, but they became completely exhausted when they tried to coördinate these two powers.[5] It is understood that

[3] *Vide,* Lombroso: After Death—What? 1909. Carrington: Eusapia Palladino and her Phenomena, 1909. Podmore: The Newer Spiritualism.

[4] *Vide,* pp. 126, footnote 197, and 503, *supra.*

[5] The control "Dr. Truman" suggested a possible explanation during the séance of January 17, 1914: "I think it would be best if our psychic was wholly oblivious of the experiment. Our psychic becomes over-anxious, and we cannot use the forces."

they are developing for this kind of work so as to furnish the psychologists of the world a "survey" of "spirit" endowment in our next series of investigations.

In the meantime the customary phenomena were taking place: the trumpet was rapped and levitated; voices of three orders gave communications. Material was at hand for physiological and physical investigation. And is not the scientific world yet divided upon Eusapia's phenomena?[6]

This report is based principally upon the following nine consecutive séances:

Date	Instruments	Object
March 14, 1914.	Telegraph instrument.	Code.
March 21, "	Kymograph, Telegraph instrument.	Physical Condition of the psychic.
March 28, "	Kymograph, Telegraph instrument.	Physical Condition of the psychic.
April 4, "	Kymograph, Telegraph instrument.	Physical Condition of the psychic.
April 11, "	Scales.	Weigh psychic force.
April 18, "	"	Weigh psychic force.
April 25, "	"	Weigh psychic force.
May 2, "	"	Weigh psychic force.
May 9, "	Telegraph instrument, Scales, Smoked paper.	Code, finger-print, touch in enclosed space.

1. Relation of the "Voices" to the Psychic's Physiological Processes.

The public has been informed through the *Proceedings*[7] of the Am. S. P. R. and through the *Open Court*[8] of the anomalous character of the voices of Mrs. Blake, of Braderick, Ohio; how they came from a "trumpet," in broad daylight, without apparent lip or throat movement, when one end of the trumpet is held at the medium's ear, in her hand lying on her lap, and even in the hands of her sitters while the medium is touching it with the tips of her fingers. And the investigators have specu-

[6] Sir Oliver Lodge, the great English physical scientist, and head of Birmingham University, Prof. Chas. Richet, the great French physiologist, Cesare Lombroso, the great Italian criminologist, besides other eminent scientists, including, I believe, some psychologists (colleagues of Mosso), defend the hypothesis that Eusapia's phenomena are produced otherwise than through the normal direct agency of the psychic.

[7] The case of Mrs. Blake. *Proceedings Am. S. P. R.*, 1913, 7: 570-788.

[8] David P. Abbott: The history of a strange case; a study in occultism. *Open Court*, May and June, 1908.

lated upon the part the eustachian tube and the bones of the head play in their production. Dr. Guthrie puts the alternatives: "The conversation is either produced by Mrs. Blake talking through her ear or by the voice of a denizen of the other world." [9]

The public has also been made acquainted with the wonderful trumpet voice of Mrs. S. which spoke by the hour to the sitters in Mr. B. O. Flower's house while the psychic was securely bound to her chair, and her hands and feet tied with tape which was tacked to the chair and to the floor. Mr. Hamlin Garland attested that "so far as touch and hearing go, Mrs. S.'s arms and feet had nothing whatever to do (in any ordinary way) with the movement of the cone [trumpet]." [10]

And the public has also learned of the marvelous independent voice of the young Miss Burton, that is said to sing simultaneously with the exquisite independent whistling,[11] and to be produced by a throat which when inspected, in the dark, two feet from that of the psychic, had the power of forming and removing, at command, an enlargement of a sublingual gland similar to the psychic's own swollen gland.[12]

As to the exact manner in which these voices are produced, there is difference of opinion. Professor Hyslop represents psychological opinion in regarding "independent" voices as automatic vocal activity for which the psychic has anæsthesia: she hears the voices, but not feeling her vocal organs in operation, or, I suppose, her breathing change, she must take them to be "independent." But, and this is the point, investigation has not yet fastened the production of the voices upon Mrs. Blake, although Professor Hyslop thinks it probable that her vocal organs operate incipiently in sympathetic rapport with them, since he, upon one occasion, was able to observe that slight action of her vocal organs accompanied the muffled sounds in the trumpet. And the sitters of the Society's séances, with possibly a few exceptions, have satisfied themselves of the actual independence of the voices produced by our psychic.

A good method of determining the facts seems desirable, and the writer believes that respiration curves and pulse curves, although rather difficult to manage, especially in conjunction with a signal-recorder and a time-marker, go a long way toward meeting this end.

The sitters in the Society's dark séances hear whispered or aspirated

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[9] *Proceedings Am. S. P. R.*, 7: 586.

[10] Report of dark séances, with a non-professional psychic, for voices and the movement of objects without contact. *Psychical Review*, 1894, 2: 152-177.

[11] *Proceedings Am. S. P. R.*, 1911, 5: 60.

[12] *Journal Am. S. P. R.*, 1909, 3: 710.

voices ringing through the trumpet with a distinct metallic quality, and whispered voices of less intensity coming out of the air, both of which are located variously anywhere in the room, and variously by the different sitters in any given instance, but usually their source is granted to be in the upper air of the center of the room, beyond the reach of the psychic. These two kinds of voices are "independent," but shall be distinguished in this report by calling them "trumpet" and "independent" voices, respectively. There is in addition to these an "automatic" voice, of a little girl, which is not whispered or aspirated, but vocalized naturally, and is produced through the psychic's vocal organs.

The relation of all these voices to the psychic's physiological processes is perhaps more than suggested by the curves reproduced on the following pages, which were taken with apparatus brought to the séance-room in the first instance for studying the psychic's physical condition during the production of séance phenomena; it was intended to record any changes in the rate or character of the pulse or in the amplitude or character of the respiration, and thus determine deviation from normal condition.

The apparatus (Fig. 8) consists of (1) a kymograph, the drum of which was turned by clockwork so as to move the smoked paper on its surface at the rate of 1.5 cm. per second; (2) a Marey tambour with a writing-finger actuated by a capsule (sphygmograph) fastened over the carotid artery by bands of tape around the neck, for recording the pulse; (3) another Marey tambour, actuated by a pneumograph fastened around the chest, for recording the respiration; (4) a vibrating time-marker recording fifths of seconds; (5) a writing-finger, controlled by the experimenter, for making signals on the drum at the moment phenomena occurred. The writing-fingers were set in a line vertically to the movement of the smoked paper on the drum and all made their records synchronously whenever the drum was started by releasing the brake.

Records were taken throughout three séances (March 21, 28, April 4). Owing to the impossibility of changing paper on the drum during a séance, all records taken during the three hours of the séance had to be made on one smoked paper, necessitating a shifting of the drum so as to gain the length of three or four circumferences, which resulted in the records tracing over each other to some extent, thus making their direct reproduction unsuitable for illustrative purposes. (Figure 9 shows the records taken March 28th and April 4th, the size being reduced to .34 diameters.) But all records are distinct, easily identified, and can be ac-

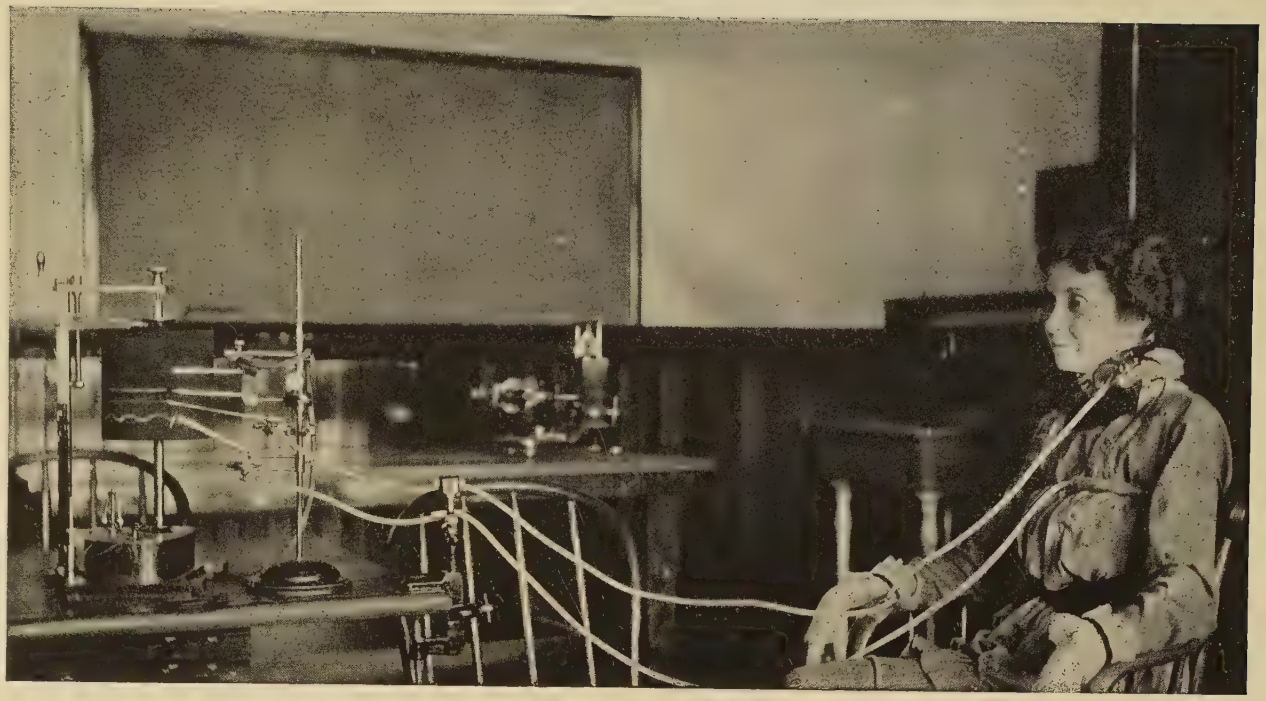


Fig. 8.— The Kymograph Apparatus.[1] (*Vide,* pp. 508, 526.)

[1] The subject is not the psychic, Mrs. Key.

curately reproduced by tracing them through carbon paper in their synchronous groups.

The following tables of records for the three séances show the number of records taken, the number of respirations or the characteristic of the respiration curve, the number of pulse beats or the characteristic of the pulse curve, the length of the record in seconds, the condition of the psychic and the phenomena present during the interval of the record. "Trance" here merely indicates that the psychic is in readiness for phenomena to occur, and not that the trance state was known to be present. "Katie" is an "automatic" voice, "General Roland" and "Dr. Truman" are "trumpet" voices, and "Professor James" is an "independent" voice.

Records of March 21.

No.	Pneumograph	Sphygmograph	Time	Phenomena
1.	3.5	12.2	10.4	Normal; silent.
2.	4 + Irreg.	12.3 + Irreg.	10.6 + 7.2	Normal: silent + speaking.
3.	Very Weak	(Trace)	19.6	Trance; quiet.
4.	4 Reg. Weak*	....	9.6	Trance, (Sitters singing).
5.	*....		2.0	Trance.
	(After No. 5 the table vibrated, Mrs. L. was touched by the trumpet, papers on the table were moved, the telegraph instrument was operated.)			
6.	(Trace)		15.8	Trance; during and after sitters' singing.
7.	5 Reg. Weak	(Trace)	16.8	Trance; Wm. James speaking.
8.	3		8.0	Trance; Katie speaking.
9.	2 Very Weak		7.4	Trance; quiet.
10.	†....		15.6	Waking; speaking.
11.			8.6	Approaching normal.
12.		(7½ resp.)	25.2	Normal; light on.

Records of March 28.

No.	Pneumograph	Sphygmograph	Time	Phenomena
1.	3.7	16	11.6	Normal; quiet. Dark.
2.	5.3+	21 + 4	15.4 + 3.2	Normal; quiet + speaking.
3.	2.2	‡....	7.6	Normal; quiet.
4.	2.8+	....	10.0 + 2.6	Following trance; quiet + speaking.
5.	2.4 Low, Even	....	7.6	Trance; quiet; sitters conversing.
6.	(Tremulous)	....	2.9	Trance; James speaking.

*Recorder pulled from the drum by strain from the tubing.
†Recorder disconnected from pneumograph by tubing pulling apart.
‡Recorder pulled from drum, even though tubing was clamped to table.

Records of March 28—Continued.

No.	Pneumograph	Sphygmograph	Time	Phenomena
7.	Irreg.	....	13.6	Trance; Truman, speaking + quiet + speaking.
8.	2+	....	9.2	Trance; Truman, quiet + speaking.
9.	Irreg.	....	12.2	Trance; Truman, speaking.
10.	3	....	14.4	Trance; Truman, speaking + quiet.
11.	1.7	....	7.0	Trance; Truman, quiet + speaking.
12.	3 Irreg. Greater amplitude.	....	14.4	Trance; Katie speaking.
13.	2 Irreg.	....	9.4	Trance; Katie speaking.
14.	Reg. + Irreg.	....	11.2	Normal; quiet + speaking.
15.	Irreg.	(Trace)	8.8	Normal; light on.

Records of April 4.

No.	Pneumograph	Sphygmograph	Time	Phenomena
1.	7.3 + 1	28.3 + 6	23.7 + 3.6	Normal; quiet + speaking.[13]
2.	1.0 + 5.7	Irreg. + Reg.	2.8 + 17.3	Normal; speaking + quiet + "No" + quiet.
3.	Irreg. + Reg.	Irreg. + Reg.	3.1 + 4.2	Normal; speaking + quiet.
4.	Irreg.	Irreg.	12.8	Trance; James speaking.[14]
5.	Irreg.	Irreg.	7.6	Trance; James speaking.
6.	3.7	14.5	10.7	Trance; quiet.
7.	Irreg.	Irreg.	18.2	Trance; Dr. Truman speaking.[15]
8.	Low	(Trace)	4.6 + 3.0	Normal.
9.	Reg. + Irreg.	Reg. + Irreg.	20.2 + 9	Normal; quiet + speaking.
10.	Irreg.	Irreg.	3.8	Normal; speaking.

Out of an aggregate of 37 records, 8 were taken when the psychic was normal before phenomena occurred, 8 when normal or approaching normal after phenomena had ceased, 1 followed trance out of which the psychic was aroused before phenomena began, 7 while the psychic was apparently in trance and was quiet, 3 while the "automatic" voice was speaking, 6 while a "trumpet" voice was speaking, and 4 while an "independent" voice was speaking.

[13] "No, it doesn't" (in reply to experimenter's question whether the apparatus interfered with the psychic's breathing).

[14] "I wish I might be able to speak more distinctly so as to make a perfect record."

[15] "We thank you for your painstaking interest and [here the record stopped] work."

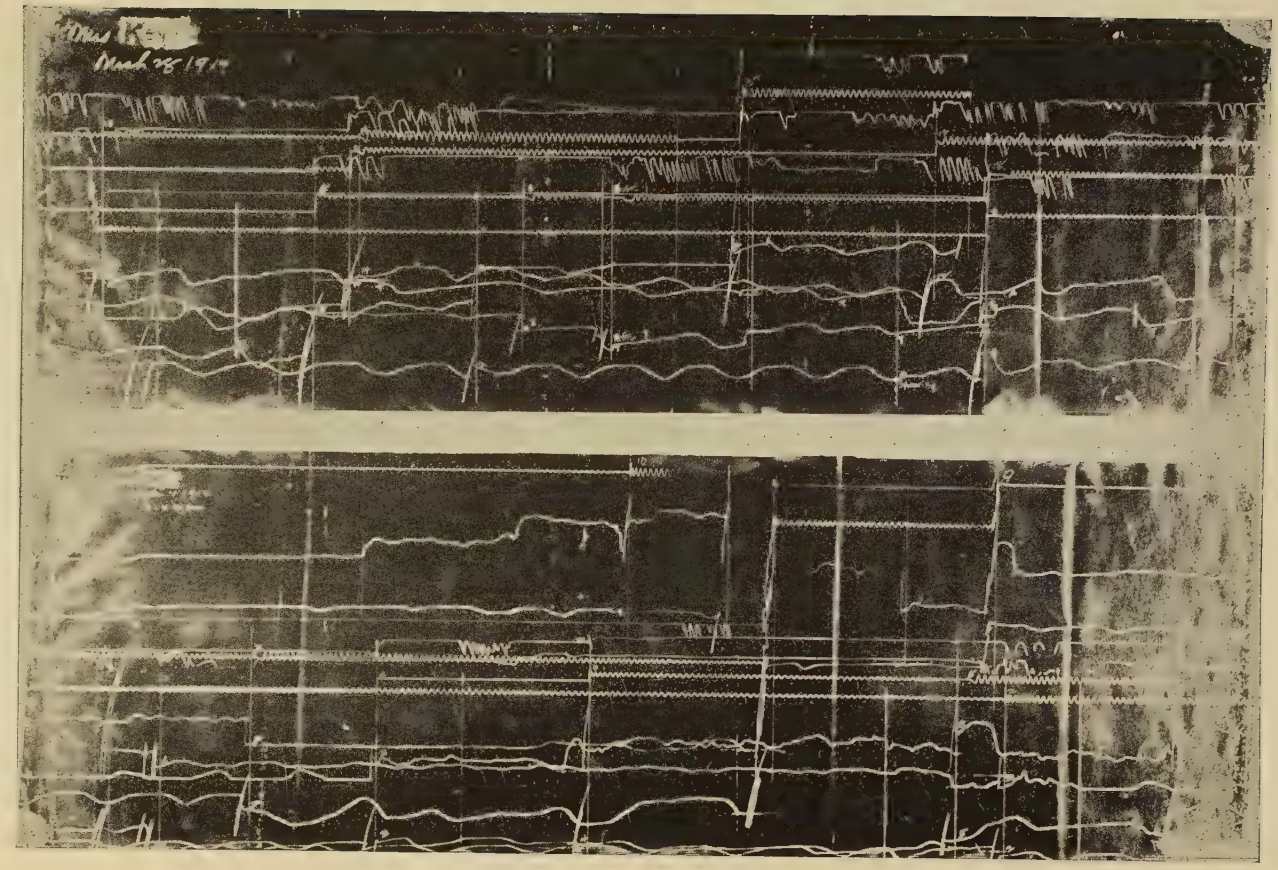


Fig. 9.— Kymograph Records, March 28 and April 4. (*Vide*, p. 508.)
(Size .34 diameters.)

It would seem that we obtained sufficient data to determine very accurately the physical condition of the psychic, as compared with her normal condition, during the production of séance phenomena, and to state whether she is in a trance state, and whether the different kinds of voices or whether the different séance personalities correlate with the same or with different physical states. As a matter of fact, our data on the rate and amplitude of respiration and pulse are exceedingly limited. We have only 7 records of both respiration and pulse from which we can calculate rate, and but one of them was taken during trance; it does not show any significant deviation from normal records of other days, but it is higher in rate of pulse than the preceding record of the same day, which, with Record 12, of March 21, indicates an acceleration of pulse during the séance; there is also indication that immediately after the séance the rate is very high (March 21), and that in about 20 minutes it subsides to normal (April 4).

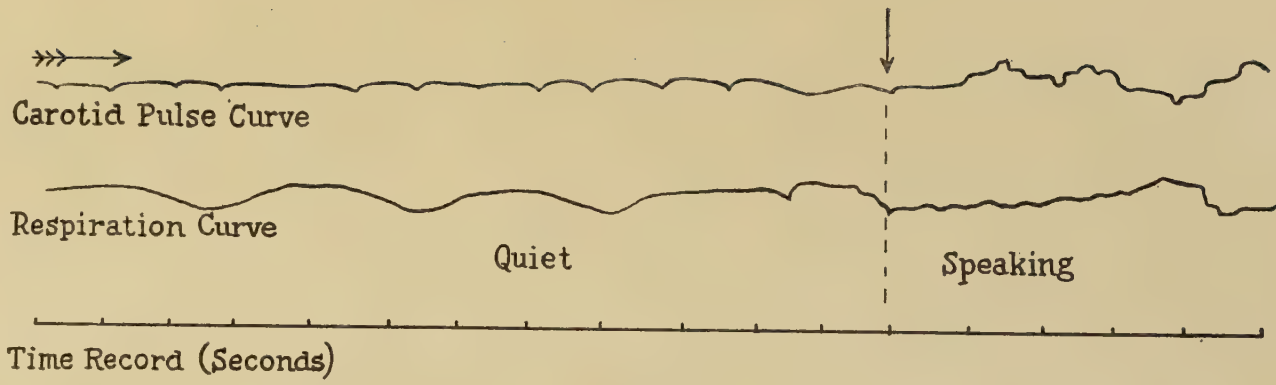
The following table gives our complete results for rate of respiration and rate of pulse:

Date	Record	State	Rate[16] of Respiration	Pulse
March 21	1	Normal	19.5	67
"	2	"	16.2	66.6
"	12	"	17	79
"	Counted just after séance closed			92
" 28	1	Normal	18.1	79
"	2	"	19	77
April 4	1	"	17.6	68.2
"	6	Trance	19.7	77.4
"	Counted 20 min. after close of séance			72

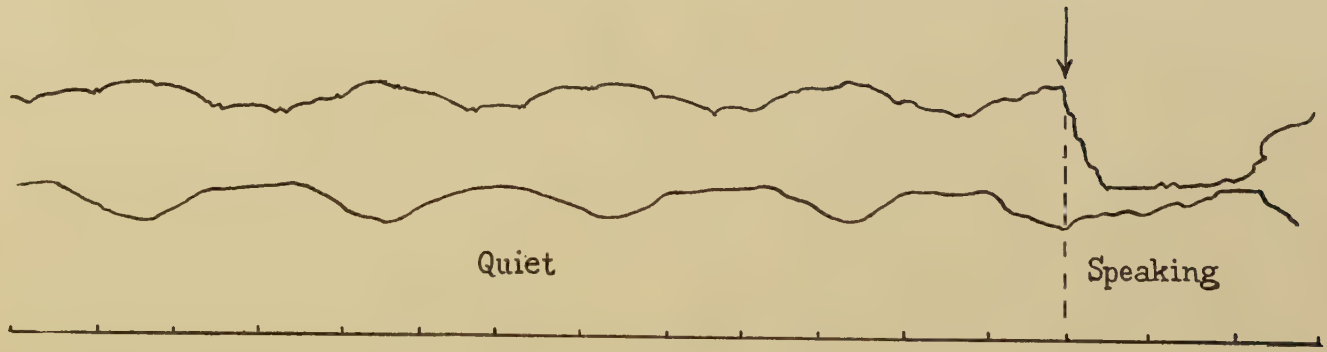
There is evidence that the psychic's pulse changes its rate considerably within short intervals of time; and the form of the respiration curves indicates a decrease in amplitude during the séance. It is probable that the psychic's physical state varies much from normal, but the question must be left to be settled by our further investigation.

The meagerness of our data for determining the physical state of the psychic during séance phenomena resulted from two causes: (1) In the records of March 21, and March 28, there were blanks, owing to one or both of the writing-fingers being pulled out of place; and (2) in all of the records taken during speaking, either of the psychic's voice or

[16] These rates are corrected from the kymograph records by multiplying by 100/105 (Time-marker beat 315 times a minute instead of 300 times).



a. March 21. Record 2.



b. March 28, Record 2.

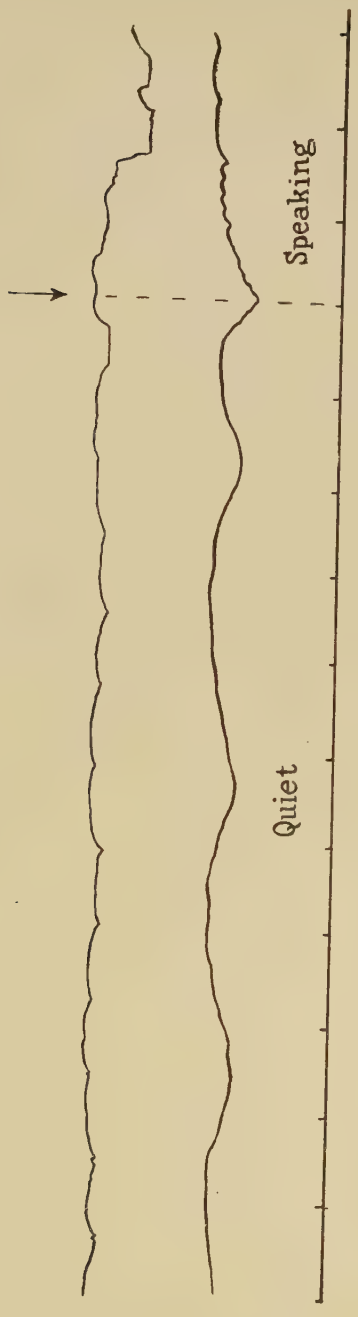
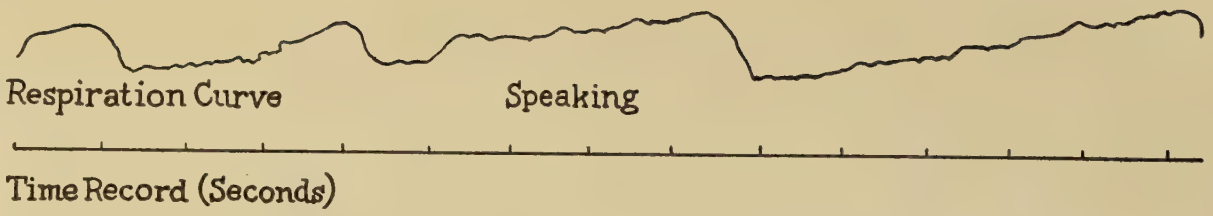
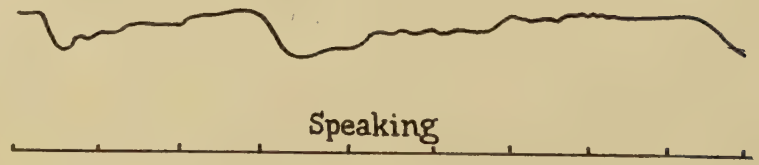


Fig. 10.—"Psychic" Normal.



a. March 28, Record 12.



b. March 28. Record 13.

Fig. 11.— "Automatic" Voice, "Katie."

of the séance voices, both respiration and pulse curves are so much disturbed that they contribute nothing to our primary inquiry.

The significance of the former cause will be noticed later in the consideration of the relation of the physical phenomena to the psychic's organism; present interest lies in the significance of the irregularity of the respiration and pulse curves during the speaking of either the psychic or the séance voices.

The nature of this irregularity can be shown by records taken when the psychic was normal; a pair of curves is reproduced from each day, in Figure 10 (pp. 512 and 513).

It is only necessary to point out that when the psychic is quiet, the curves are regular and rhythmic; when the psychic speaks, the pressure on the capsule over the carotid pulse becomes irregular owing to the vocal organs interfering with the bandage around the neck which holds the capsule in place, and the tracing becomes so irregular as to conceal the pulse—becomes in effect a voice-curve; the respiration changes from rhythmic to irregular, inspiration is quicker and deeper (shown by the downward stroke in the lower curves of the pairs) and expiration is slower, and intermittent or broken by the surd consonants (shown by the upward stroke which changes from a smooth compound curve to an irregular line, sometimes almost straight).

The same irregularities occur in the respiration and pulse curves of the psychic when the "automatic," "trumpet," and "independent" voices speak in the séance.

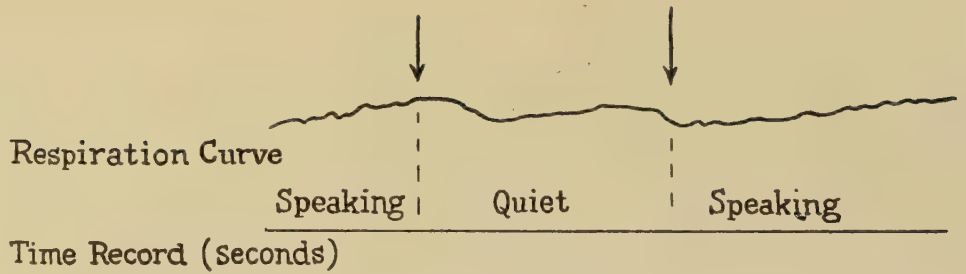
Only the respiration curves can be shown for the "automatic" voice, which, as the sitters know, uses the psychic's vocal organs. See Figure 11 (p. 514).

During the last record "Katie" said: "We are anxious to have the truth demonstrated."

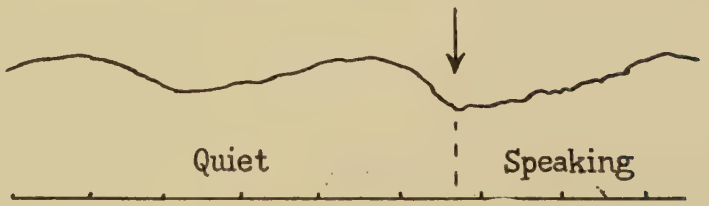
For the "trumpet" voice, we may select the following four records: Figure 12 (pp. 516 and 517).

In the first record "Dr. Truman" said: "We are all very interested in this experimentation; as much as yourselves. . . ." During the third: "Don't be discouraged; it requires much patience; much perseverance. Without faith to sow you could not reap." And during the fourth: "We thank you for your painstaking interest and work." (This was the last record of the séance voices). His speaking was deliberate, in phrases, dignified.

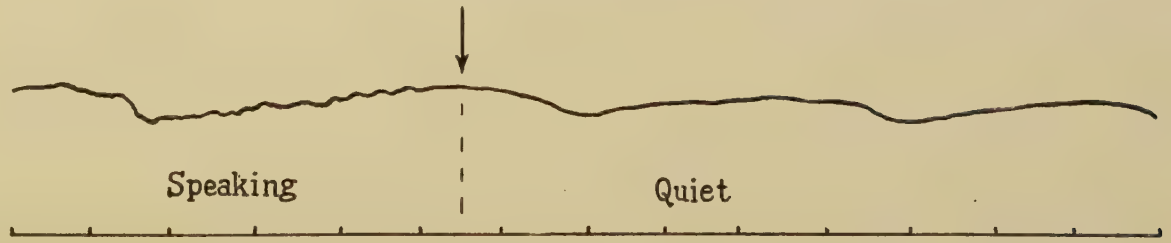
For the "independent" voice, perhaps the following will serve: Figure 13 (pp. 518 and 519).



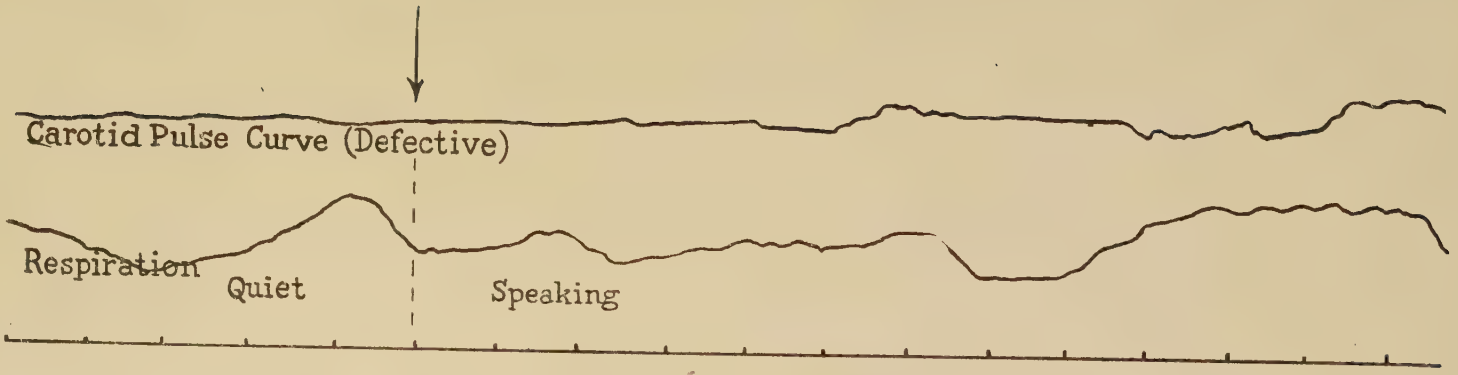
a. March 28, Record 7.



b. March 29, Record 8.

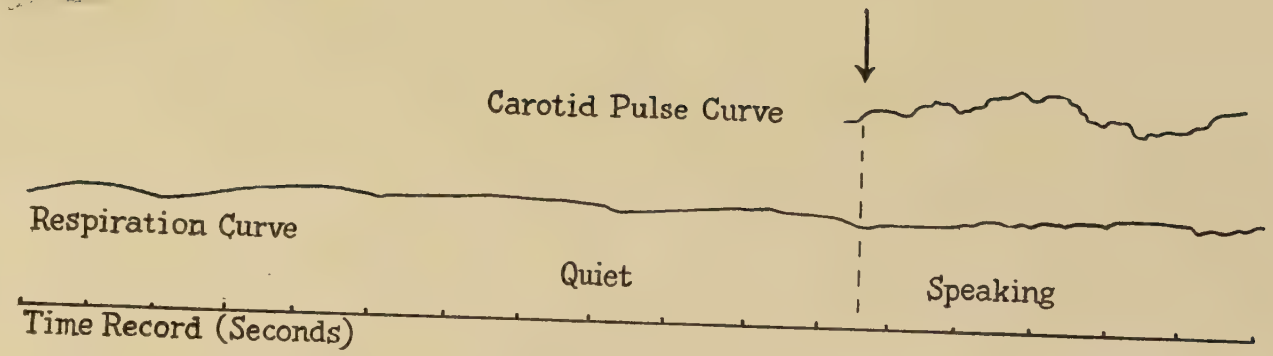


c. March 28. Record 10.

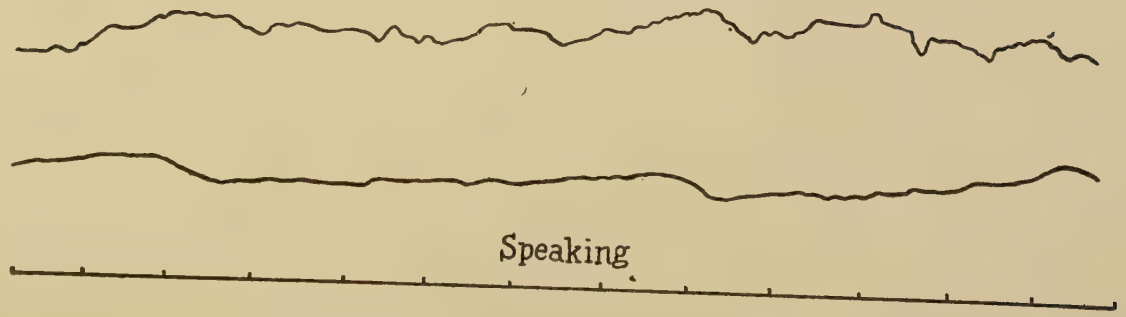


d. April 4. Record 7.

Fig. 12.—"Trumpet" Voice, "Dr. Truman."



a. March 21, Record 7.



b. April 4, Record 4.

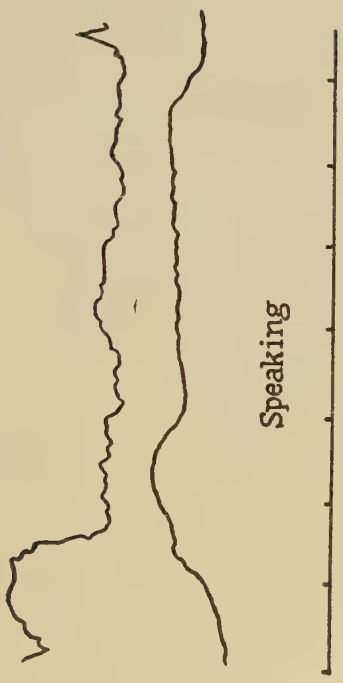


Fig. 13.—"Independent" Voice, "Professor Wm. James."

The pulse curve in *a*, above, was obtained accidentally; at that point there were two thicknesses of paper on the drum caused by the sheet lapping over for pasting, which brought the surface in contact with the dislodged writing-finger.

During the second record, "Professor James" said: "I wish I might be able to speak more distinctly so as to make a perfect record."

Now, it is well known that many subjects in the laboratory show involuntary movement of the tongue [17] and of the larynx [18] when they read silently, think [19] words, or even hear words or songs; and although Professor Hyslop [20] was able to observe "action of Mrs. Blake's vocal muscles very distinctly when [trumpet] communications were going on with Dr. Guthrie" and he notices the coincidences between this vocal action and the muffled sounds in the trumpet, and although the impression made upon him was that "the evidence was unmistakable that Mrs. Blake's vocal muscles were used in producing the sounds," [21] he nevertheless appears inclined to regard this action as possibly merely sympathetic: "That her vocal organs act at least sympathetically with the voices in the trumpet there is no doubt, but that they cause the phenomena is not proved by the imitative experiments [with a trumpet] recorded." [22] The same implication is to be found in the reports of the investigation of Miss Burton by two physicians, Drs. Smyth and Hamilton: "We were permitted to examine [by feeling with the hand] the larynx and lips of the psychic while the [independent] singing and whistling was in progress. . . . Without being able to detect sound issuing from her throat or mouth, the psychic's larynx vibrated in sympathy with the deep contralto tones produced in the trumpet several feet away." [23] Professor Hyslop verified the fact of vibratory and muscular movement in the throat synchronously with independent singing and whistling, in his investigation of Miss Burton. This action he says, in the introduction, "is at least sympathetic and would in most cases be accepted as conclusive evidence of an adequate explanation of the phe-

[17] Wyczoikowska: Theoretical and experimental studies in the mechanism of speech. *Psychological Rev.*, 1913, 20:448-458.

[18] Curtis: Automatic movements of the larynx. *Am. Jr. Psych.*, 1899-1900, 11:237-239.

[19] Stricker: Die Sprachvorstellungen, p. 16.

[20] *Op. cit.*, p. 721.

[21] *Op. cit.*, p. 721.

[22] *Op. cit.*, p. 602.

[23] Experiments with trance phenomena. *Jr. Am. S. P. R.*, 1909, 3:707.

nomenon. But several circumstances make this an issue still to be determined." [24]

This interpretation of our results also occurred to the President and others of our Society, and the records that were taken from normal university students by the writer to illustrate, if not to prove, the improbability of the "sympathetic activity" theory, may be of interest here.

The same apparatus was set up in Room 397 in the Psychological Laboratory at Stanford University, and records were taken from two young women who were attending one of the writer's classes.

The records from Miss Flatau will illustrate the facts. As shown in the curves above, the carotid pulse curve is the upper, and the respiration curve is the lower member of each pair. The first pair records the changes caused by *speaking aloud*: "I wish I might be able to speak more distinctly"; the second pair records the changes caused by *whispering* the sentence so that it could be heard by a companion; the third, by *whispering with closed lips* (she could feel the vocal organs move); the fourth, by *inner recitation* of the sentence (with attention upon the pronunciation rather than upon the meaning of the words). Figure 14 (p. 522).

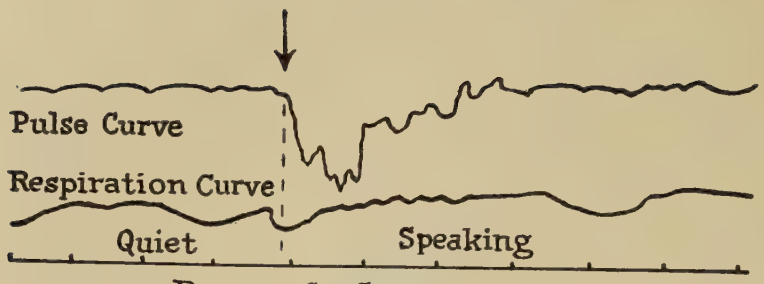
Records 1 and 2 show the characteristic in expiration common to the curves of the séance voices, as well as of the psychic's voice, which results from the use of the air in the lungs by the vocal organs for speech. Records 3 and 4, which were taken with 1 and 2 at the same sitting, with the instruments in the same adjustment, show that "sympathetic action" of the vocal organs, even when intentionally induced as in Record 3, is but slightly transmitted by our instruments and then only in the carotid pulse curve.[25]

If the reader will imagine that he hears the sentence pronounced in a very dignified manner, and permits his vocal organs to act "sympathetically" with the imagined voice, he will find that this sympathetic activity is independent of his respiration and occurs synchronously with inspiration as well as with expiration.

The fact then appears to be that the peculiar characteristic in the respiration curves is caused by the breath being conserved in expiration, by the constricting movements of the vocal organs, and that articulated

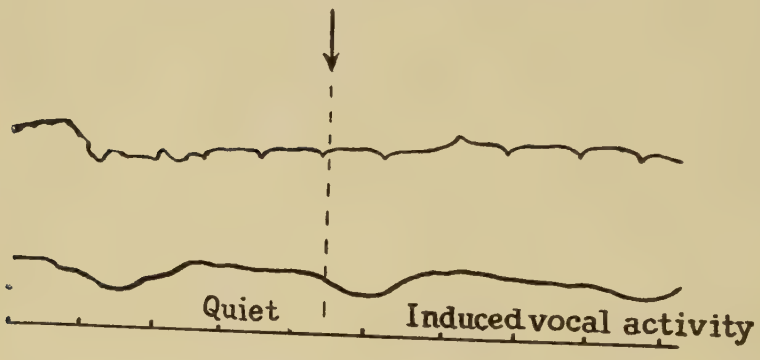
[24] A case of hysteria. *Proceedings Am. S. P. R.*, 1911, 5:43.

[25] The experimenter is aware of great variation between individuals with respect to the amount of involuntary activity of the vocal organs during silent reading or recitation; yet he suspects that further experiments will show that the present apparatus is not well adapted to test its presence.

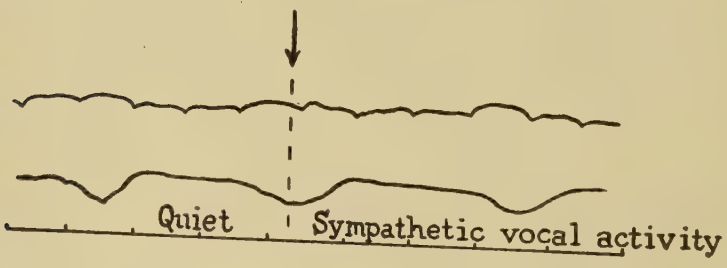


Record 1. Speaking aloud

Record 2. Speaking in a whisper



3. Whisper with closed lips



4. Inner recitation

Fig. 14.— Records from Miss Flatau.

sounds, either vocalized or whispered, must take place. The hypothesis that the restrained respiration may be due to the control of the diaphragm and intercostal muscles, as in the case of strained attention, and that therefore movements of the vocal organs during restrained expiration not being accompanied by constriction of the glottis, need not issue in articulated sounds, may be dismissed as entirely improbable, since the assumed control affects equally inspiration and expiration, while only expiration was restrained. And if a critic were to still insist that the peculiar circumstances of the séance may after all necessitate serious consideration of this last hypothesis, it can be pointed out that partial suppression of respiration by the assumed control could not account for the irregular form of the expiration curve, which conforms precisely with what one would expect on the assumption that the breath was being used for pronouncing syllables which make unequal and irregular demands upon it; in other words, the expiration was not only partially suppressed but it was intermittently stopped by the production of certain consonantal sounds—the mutes.[26] If the reader will turn back to the respiration curves and note the minute irregularities in all of the expiration curves taken when there was speaking, he will realize the force of this point.

So far as our evidence goes it is wholly in the direction of the hypothesis that the séance voices are caused by the vocal organs of the psychic. She herself may be anæsthetic for her vocal activity and may regard the voices as independent; at any rate, as will be shown later, the hypothesis of conscious fraud seems highly improbable.

Further work by the use of the kymograph and the many useful instruments for recording upon it is projected for next year's investigation. The movement of the tongue and larynx can be directly recorded, and true "word curves" of the séance voices can be compared with those of the psychic. Pulse and respiration can be determined for states accompanying the various voices, and the question as to whether there is trance or regional anæsthesia can be settled.

Other scientific instruments may be used to bring the question of the relation between the séance voices and the psychic's vocal organs to a definite solution: the dictaphone or telegraphone, with the transmitter

[26] The fact of unequal demand of syllables upon the breath may be verified by pronouncing for contrast, hat, pat; what, bought; kick, whizz, etc. It also happens to be used in Webster's International Dictionary in the definition of "mute" used as a noun in phonetics: "An element of speech formed by a position of the mouth organs which stops the passage of breath;" as, *p, b, t, d, k, g.*

at the lips of the psychic for recording any sounds issuing from them, and a duplicate in the room or in the trumpet for recording the "independent" voice, would enable the experimenter, by comparison of records, to determine whether "independent" voices occur at all without the accompaniment of sound from the psychic's vocal organs, and would throw light on the relative amount of the sound contributed by the psychic's vocal organs, helping to determine whether all the sound is produced by them. These instruments could record synchronously with the laryngeal and respiration recorders so as to insure the proper adjustment of the transmitter at the mouth during "independent" speaking. Emulsion could be placed in the trumpet for analysis of the air after voices speak through it, to determine whether it is free from traces of the psychic's breath which may have been prepared for easy detection, and the results compared with the analysis of emulsion exposed to the general air in the room in another trumpet which is under precise control.[27]

2. Relation of the "Physical Phenomena" to the Psychic's Body.

Upon the hypothesis that the séance voices are produced by the psychic's organs of speech, it would be difficult to explain the function of an aluminum trumpet, unless it is to augment the sound of the voice, or to direct it and give it a location, by speaking through it. But this would involve the psychic's free handling of the trumpet, which, it is claimed, is not done.

Hamlin Garland, in a sitting with Mrs. S., witnessed the use of the trumpet, for rapping, touching, and speaking, when it was placed on a table "entirely out of reach of the psychic." [28] who, moreover, was tied

[27] Less certain tests could be employed; such as, (1) a solid wire or willow frame to clamp over the psychic, enclosing her body from the waist up, or to screw down on the floor over the psychic and her chair, leaving the trumpet outside; (2) a cage for the trumpet; (3) water of an unknown color to be held in the mouth during "independent" speaking; (4) the mouth sealed with surgeons' tape; (5) small paper bags tied over the psychic's hands; etc. But they fail to reveal the method of the production of the voices, and, besides, there are well-known ways to circumvent them by irresponsible but ingenious secondary personalities, which deprive them of evidential value: the reaching-rod taken from the waist and handled inside the cage would nullify the first test; a second concealed trumpet, or even the "resonance" of the "controlled" trumpet, the second; a collapsible cup to hold the water while speaking, the third; displacement of tape, the fourth; removal, the fifth; etc.

[28] Garland: Sounds, voices, and physical disturbances in the presence of a psychic. *Psychical Rev.*, 1:228.

hand and foot to her chair with tape the ends of which were nailed to the floor; again, in a later sitting,[29] while the psychic was even more securely tied down hand and foot, the tape nailed to her chair and to the floor, and while he held a thread tied to one of the psychic's wrists, and Mr. Flower held a thread tied to the other wrist, so as to be able to detect the least movement of the hands, the trumpet, which had been placed on the floor 25 inches from her right hand, was used for speaking; and again, while Mr. Garland had one hand on the psychic's left arm, and Mr. Flower one hand on her right arm, and their other hands were upon her head, the trumpet, which had been placed on a table 28 inches from the psychic's left hand, was levitated and deftly manipulated.[30] Mr. Garland concluded: "So far as the senses of touch and hearing go, Mrs. Smith's [the psychic's] arms and feet had nothing whatever to do (in the ordinary way) with the movement of the cone [trumpet]." [31]

While Dr. Hyslop was holding both of Miss Burton's hands in his left and grasped the large end of the trumpet with his right hand, the trumpet was moved and jerked without any apparent means. He writes: "I was exceedingly careful to observe the behavior of the hands. Both times, when the trumpet was jerking, Miss Burton's hands were absolutely passive during the whole performance and no motion of them whatever was detectable, except when the trumpet was perfectly still. . . . It was physically impossible for her feet to get at it, as I took care to observe at the end of the evening's work. . . . I offer no explanation of the facts." [32]

Our sitters have also taken precautions to have Mrs. Key tied to her chair, and the trumpet out of her reach, in séances during which "trumpet voices" spoke freely, and the general impression which they entertain is that she does not touch the trumpet, or any of the other objects that are moved in the production of séance phenomena.

Now, it will be remembered that one cause for paucity of kymograph data for studying the physical condition of the psychic during the séance was the blank records resulting from one or both of the writing-fingers being pulled away from contact with the smoked paper on the drum. They occurred during the séances of March 21, and March 28, and it is just possible that they throw some light upon the manner in which the physical phenomena are produced.

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[29] Report of dark séances, with a non-professional psychic, for voices and the movement of objects without contact. *Psychical Rev.*, 2: 170 ff.

[30] *Ibid.*, 170-3.

[31] *Ibid.*, p. 174.

[32] *Proceedings Am. S. P. R.*, 1911, 5: 562 ff.

On March 21, after Record No. 5, and before Record 6, physical phenomena occurred: the table vibrated, a sitter remote from the psychic was touched by the trumpet, papers on the table were moved, and the telegraph instrument standing on the table was operated. Now, by the 4th record the pulse recorder, and by the 5th the respiration recorder, had left the drum; by the 7th record the latter returned. The two recorders must have been pulled independently of the other recorders, since, while they were out of commission, the time-marker and the other signal-recorder, which were clamped to the same standard, were still in function. This could be done only by changing the strain upon them by shifting the rubber tubing connecting them with the psychic. The recorders had been relieved from much of the weight of this tubing by supporting the spans (about six feet long) between the table and the psychic upon the seat of a chair drawn up to the end of the table. Change of strain would necessitate rather free movement of this tubing in order to raise it from the support of the chair and could scarcely be effected without the psychic leaving her chair, and, consequently, might have taken place in the following manner: After Record 3, during the singing of the sitters, the psychic released herself and rose to her feet, by Record 5 she had stepped slightly forward toward the trumpet and the front of the table where the telegraph instrument was placed, by Record 6 the phenomena had been produced and she was standing by her chair, by Record 7 she had returned to her seat, relieving the respiration recorder of sufficient strain to begin recording again. This interpretation may be defective in some of its details, but that it is essentially correct seems to be borne out by other facts to be noticed later.

One corroborating circumstance is that on the side of the drum which was next to the telegraph instrument when that instrument was operated, between Records 5 and 6, there is a smudge (Fig. 15) on the kymograph paper caused by the removal of a square inch or more of lamp-black, such as might be made by a sleeve or other fibrous surface moving in a downward direction. It is scarcely necessary to state that this sheet of records was carefully guarded until it was "fixed" in a bath of shellac immediately after the séance, and that the smudge could not have been an accident in manipulation; while it was on the kymograph it was beyond the field of the experimenter's hands and entirely safe from his clothing.

On March 28th, the tubing was held in a clamp fastened to the edge of the table (as is shown in Fig. 8), but in spite of this greater security the pulse-recorder was thrown out, after Record 2, by the tubing being

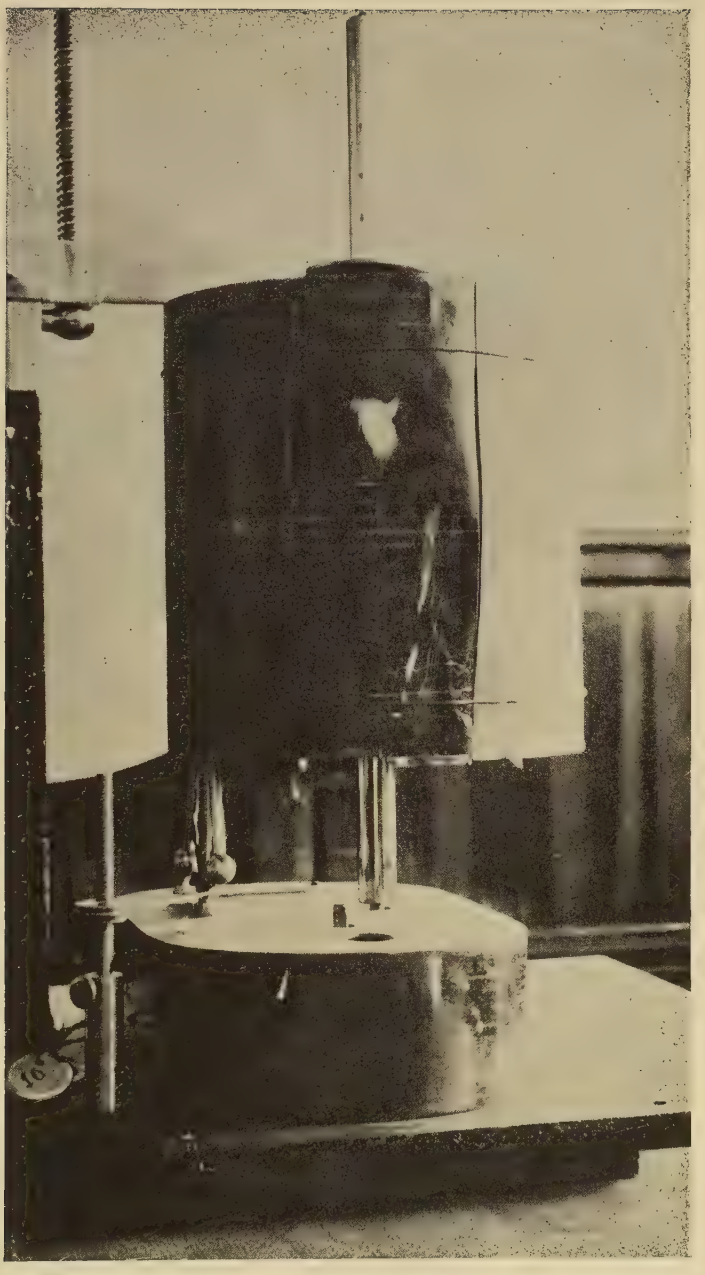


Fig. 15.— Smudge on Kymograph Record. (*Vide*, p. 526.)
(Size .42 diameters.)

pulled rather energetically. In view of "Katie's" statement, before the séance closed, that it was thought that neither of the writing-fingers had been recording during the speaking of the "independent" voices, the inference suggests itself that she perhaps knew of circumstances calculated to put both recorders out of commission again. In the next séance, April 4th, precautions were taken to clamp the tubing still more securely, and the experimenter got permission after each record to use a weak ruby flash-light to inspect the apparatus, thus insuring complete records. Concerning the coöperation of the "séance personalities" more will be said later.

Our next endeavor was the classical experiment of measuring "psychic force." It will be remembered that Sir William Crookes,[33] in 1871, measured, in daylight, by means of self-registering spring balances, the "psychic force" exerted by the American medium D. D. Home, when the latter was in contact with, and when he was at a distance of three feet from, the mahogany board upon which the force was exerted; that he represented pictorially the variation in the force exerted, by means of a traveling smoked glass and a writing-finger attached to the indicator of the balances. And in Turin, in the early part of 1907, the assistants of Mosso (Foa, Herlitzka, Aggazzotti)[34] repeated the experiment, in an improved form, with Eusapia Palladino. After "John," Eusapia's "control," had been given an opportunity to press a telegraph key in a closed box, and he did so only after forcibly removing the cover, Eusapia explaining that had the cover not been of cardboard but of woven texture he could have operated in the enclosed space, the experimenters provided a vessel of water, covered by a rubber capsule, which was placed in a box over the top of which a cloth cover was tacked. The water was in connection with a manometer which would transmit force exerted on the capsule to a writing-finger in contact with a kymograph drum, by which the amount of force, its variability, and the length of time it was exerted, would be pictorially represented. The capsule was coated with lamp-black to show the manner of contact. "John" tore the cloth cover in exerting the "force" on the capsule.

During four séances (April 11, 18, 25, May 2) our "séance per-

[33] Crookes: Researches in the Phenomena of Spiritualism. London, 1874, 14-17, 33-42.

[34] Podmore: Newer Spiritualism, 101 ff.; Lombroso: After Death—What? 76 ff.; Carrington: Eusapia Palladino and Her Phenomena, 100 ff.; and *Annales des Sciences Psychiques*, 1907.

sonalities" were given an opportunity to exert "force" on a platform spring-balance which was provided with an indicator that marks the maximum position of the scale-pointer in its swing around the dial. The platform is square, and was provided with white cardboard covers (6 in. by 6 in.) which fit loosely yet closely over it, coated with lamp-black to record the manner of contact. The covers were not seen by any of the sitters or the psychic, were brought prepared to the séance and were put on and taken off in the darkness, usually during the singing of the sitters.

In these four séances the program was:

(1) To get psychic force exerted upon the scales by several "séance personalities," especially "Colonel Roland," one of the regular controls, "Sir William Crookes," [35] who would be especially interested in his own experiment, and "Professor Wm. James," who has shown great interest in all our investigation by scientific instruments.

(2) To get "International" or "Morse" "code" on the telegraph instrument (see Fig. 16), which records automatically on a ribbon of white paper run through the apparatus by clock-work, and which had often been operated, even by "Phillips," the wireless hero who went down on the Titanic, but had failed to record "code."

(3) To have one of the controls, "Colonel Roland" or "Dr. Truman," who customarily use the trumpet, speak.

The third phenomenon is not special, since it occurs at almost every séance; and is mentioned here to indicate that care was taken to get it *after* "psychic force" should be exerted upon the scales. The second was unique only in insistence upon "code," and was subordinated to the first.

The first séance was a blank.

In the second séance, the trumpet was thrown over but was not used for speaking. "Katie," the control who speaks in "automatic" voice, said in reply to the experimenter's inquiry that "Sir Wm. Crookes," [36] and "Professor Wm. James" are here. Experimenter: "We may hope to have them, each of them, exert force on the scales, may we?" "Katie": "Yes, sir." The scales are manipulated, and the experimenter asks if he may use his ruby light to read the dial. "Katie": "No force was exerted on it." Experimenter: "Who touched the scales?" "Ka-

[35] The experimenter was under the impression that this eminent scientist was dead. Upon visiting the library (May 25, 1914), he finds that Sir Wm. Crookes is still active and publishing scientific papers, and is now the honored president of the Royal Society of London, of which he was elected a Fellow in 1863.

[36] See footnote 35.

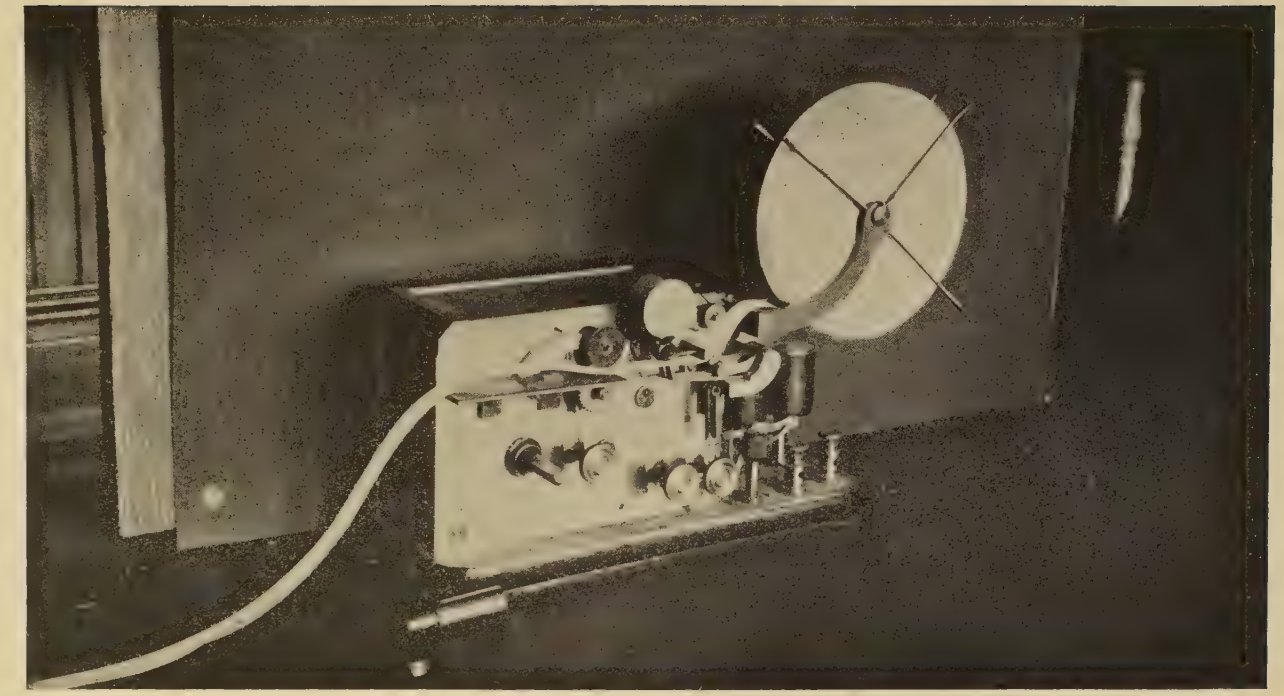


Fig. 16.— The Self-Recording Telegraph Instrument. (*Vide*, p. 528.)
(Size .25 diameters.)

tie": "Dr. Truman was the one who touched the scales. . . . If they should touch the apparatus, could you account for the Secretary's hands?" Experimenter: "Yes." The scales are rather violently manipulated, the cover falling off upon the table. Experimenter is given permission to use his light to read the time and the dial: 4:40, 5¼ lb. "Katie": "Dr. Hodgson exerted the force; he said he was too heavy; he exerted too much strength; he knocked something off the top of the apparatus."

There was no operation of the telegraph instrument, and no trumpet voice, although the trumpet was thrown down.

The cover which had been on the scales bore only some fingerprints of the experimenter's left second finger, on one corner, made in locating it after it was thrown off. *The force was applied after the cover had been removed.* Apprehension of the "séance personalities" and disquiet of the psychic will be discussed later.

In the third and fourth séances the psychic's hands were not inclosed in paper bags, but she was tied to her chair in the customary perfunctory manner.

During the third séance there was considerable jarring of the floor, and the scales kept up an almost incessant rattle. The séance was a blank. The trumpet was thrown down, picked up, rapped in the air, set deftly on the table. "Katie" said it was "Colonel Roland" who manipulated the trumpet, that he was sorry that he toppled it over, and that President Booker would understand why the trumpet fell. More of apprehension later. "Katie" said the "forces" were present and were trying to exert force on the apparatus, but that they would have to give it up. The trumpet was not handled with bare hands.

In the fourth séance three "personalities" exerted "force" upon the scales, and "Colonel Roland" spoke through the trumpet. There was no operation of the telegraph instrument. According to custom the experimenter got permission after each manipulation of the scales to read the time and the dial:

(1) 4:32, 5¼ lb. by "Colonel Roland," with his right hand.
(2) 4:45, 1¾ lb. by "Professor James," with his right hand.
(3) 4:52, 3¼ lb. by "Dr. Hodgson," with his left hand.

"Katie" told us who exerted the force and what hand was used. She said "James" was very much pleased with his performance, and that he exerted all the pressure he could at this time. "Colonel Roland" said that "Dr. Hodgson" did not wait long enough to gather sufficient force to make his record a personal expression. For the third record there

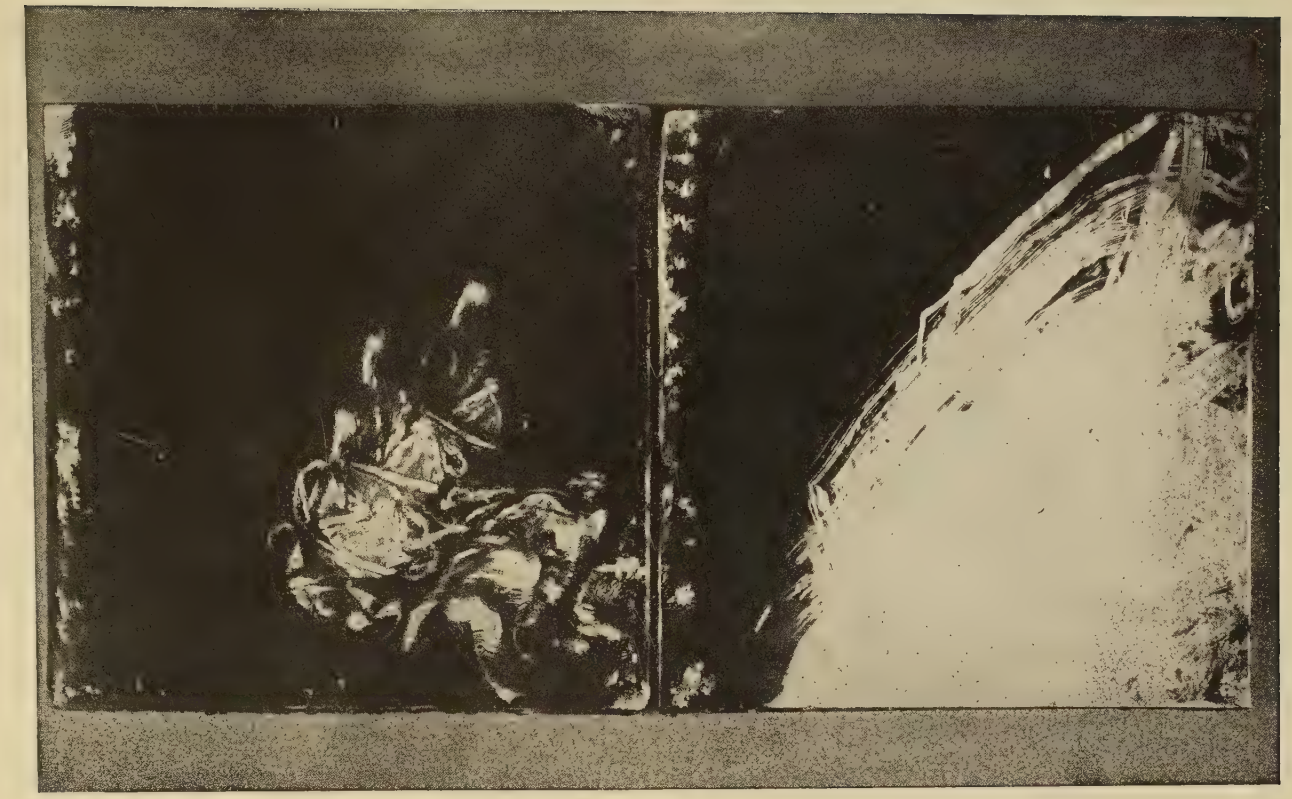
was no cover on the scale-top, but it is painted black and was prepared by a very thin film of talcum powder which showed that the force was exerted through a fabric of some kind, not a bare hand or fingers.

This brings us to the important consideration in this topic. Could the psychic's body have been used directly, and in no occult way, in the production of these physical phenomena? All the evidence we have points that way. The contact with the cover of the scales was an ordinary kind of contact, and was applied in just such a manner as a person would use who wished to avoid leaving his mark of identification. Force was applied to the scales four times. The record of the first contact was lost because "Dr. Hodgson" knocked off the cover before he exerted the force. The record of the fourth could not be preserved and reproduced, but the talcum powder was competent to show that force was applied through fabric. The second and third records are shown in Figure 17.

The second cover (a) shows that "Colonel Roland" carefully wiped off the lamp-black from the corner of the cover next to the psychic, before he exerted his force; and test with graphite showed that he did not use a bare hand.

The third cover (b), which was exchanged for the second during the séance, shows that "Prof. Wm. James" took the precaution of exerting his force through woven fabric wrapped around or held in a left hand. (See Figure 18.) The force was applied upon the side of the platform which on the immediately preceding cover was carefully wiped off; and the query naturally arises as to whether he suspected that he was using the cleaned cover. The fabric he used is of fine weave, undoubtedly of ribbed silk or lisle-silk, such as is used in the manufacture of fine ladies' gloves or stockings; accurate count shows an average of 13 ribs to the quarter inch, and of 14 threads transverse to these to the quarter inch.

Whether "Colonel Roland" used the same piece of fabric to clean off his scale-cover, cannot with certainty be determined, although traces show 13 ribs to the quarter inch, for he may have used one just like it (*e. g.*, the other glove); but it is certain that he used *sooty fabric* in handling the *trumpet* through which he spoke after his experiment with the scales, for the trumpet had been carefully polished and it now carries irregular patches of lamp-black at grasping-distance from the small end, and it does not show contact of the bare hand or fingers, except those accounted for, which it is competent to do. The fabric is woven and is undoubtedly that used by himself in handling the scales the moment before, for "Professor James" exerted his force on the scales after the lamp-black



(a) (b)

Fig. 17.— Record of Contact on the Scale-Covers. (*Vide*, p. 530.)
(Size .53 diameters.)

was left on the trumpet—the only séance voice to speak later being "Katie."

The experimenter may now take the last step, by going back to the first séance (March 14th), of which nothing has yet been said, and thus relate the beginning and end of his halting investigation. He has just noted that the surface that touched the trumpet is the same surface that touched the scales. Could this surface have been manipulated directly by the psychic's body? Before that first séance, the program for which involved a message from beyond the veil given in a language familiar to a departed personality but unknown to the psychic and sitters (except the experimenter) by the use of a self-recording telegraph instrument, the experimenter, noting the dull imprint made by the recording mechanism, procured from a neighboring shop some *printer's ink,* and a thin layer of this ink was left on the key for the purpose of recording the kind of contact. After the séance, during which the psychic was tied to her chair as usual, and trumpet voices had spoken, printer's ink was found (1) on the *trumpet* in three spots in a position convenient for grasping it between the thumb and the first two fingers of the right hand, and (2) on the thumb and the first two fingers of the *psychic's* right hand. The telegraph instrument had been taken in charge immediately after the séance, by the experimenter, and the remaining ink removed with benzine, so that there was no opportunity for the psychic to come in contact with the ink after the light was turned on. The most natural explanation is that the psychic's hand was used in touching the telegraph instrument and the trumpet.

A bit of corroboratory evidence for direct manipulation is afforded by one of the Society's séances (January 31, 1914), at which Mrs. J. sat as psychic. Mrs. J., it must be noted, sat a couple of times for the Seybert Commission, in 1885, and was one of the few mediums to whom that commission referred in complimentary terms.[37] In our séance, the table, papers, telegraph instrument, were rather violently handled. During one interval just after the telegraph instrument had been furiously "operated," the medium apparently clapping her hands meanwhile to show the independence of the phenomenon, President Booker caught her hands which had been placed on his knees, and called loudly for a repetition of the telegraph operating, announcing to the other sitters the control of the psychic. No tapping occurred. Obviously, the medium a few minutes earlier may have been patting some bare surface of her body,

[37] Preliminary Report. University of Pennsylvania, Philadelphia, 1887.

perhaps her face, with one hand while producing "physical phenomena" with the other.

The final séance (May 9th) was a blank. Although the trumpet was thrown down a couple of times, and the table was hauled out half a foot and shoved part of the way back, and metallic and non-metallic raps were produced, we did not get any of the proposed phenomena:

1. Taps as rapidly as possible on the telegraph instrument in the open air.
2. Taps on same, in a locked paper box.
3. Thumb-prints on either
   a. The white paper lying by the telegraph instrument, in the open air, or
   b. The smoked paper lying on the scales in a closed closet.

"Katie" said that "Colonel Roland" and "Dr. Truman" with others were present, and that shoving the table was incidental to getting force to do what was wanted. Misuse of force will be spoken of later.

Thus far, no "physical phenomena" have been produced which cannot be most easily explained by the direct and non-mysterious use of the psychic's body. The rope-tying feats of the Davenport Brothers,[38] Eva Fay,[39] Kellar,[40] and others,[41] do not permit one to attach any importance whatever to the tying of the psychic to her chair.[42] And present knowledge of anæsthesia for automatic phenomena,[43] and amnesia for somnam-

[38] Podmore: Modern Spiritualism, vol. II, pp. 55 *et seq.* Also, Evans: The Spirit World Unmasked.

[39] Abbott: Behind the Scenes with the Mediums, pp. 286 *et seq.* Also, Truesdell: Bottom Facts, p. 238. And Maskelyne in *Pall Mall Gazette,* April 18, 1885.

[40] Abbott: *op. cit.,* 284.

[41] Truesdell: *op. cit.,* 228 *et. seq.*

[42] All the noted mediums who have used rope or tape as alleged control of the body, have met disaster in such a way as to cast discredit upon any investigation in which such a control is relied upon. For an example, the Davenport brothers were driven out of Liverpool, Huddersfield, and Leeds (England), because they refused to proceed after a certain knot was used on their wrists, which, according to a medical examiner, did not endanger circulation (Podmore: Modern Spiritualism, 2:60). The insecurity of their bonds was revealed in Ithaca, N. Y., by the students of Cornell University, when they flashed on lights at the moment phenomena were occurring and the brothers were seen by the audience to be "dodging about the stage brandishing guitars and playing tunes and waving at the same time tall poles surmounted by phosphorescent spook pictures." (Herman: *Cosmopolitan Magazine.* Quoted by Evans: The Spirit World Unmasked, p. 144).

[43] Anæsthesia for automatic phenomena may be illustrated by the "automatic writing" of a normal subject. The hand writes intelligently, but the subject is conscious of neither its movement nor its communication. Miss Burton was seen to put her anæsthetic hand up to the side of her face upon which she exclaimed

Fig. 18.— Fabric Imprint Made by "James'" "Right Hand." (*Vide*, pp. 530, 539.)
(Size almost natural—.93 diameters.)

bulistic phenomena [44] makes these findings entirely compatible with the honesty and sincerity of our psychic.

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that a spirit hand was touching her face (*Jr. Am. S. P. R.*, 4: 55). Untying and replacing bonds or stretching beyond them, and even providing and concealing apparatus, such as reaching-rods, collapsible cups, silk gloves, phosphorescent materials, etc., to be used later in the séance, may at times be automatic phenomena, beyond the knowledge or control of the psychic. It is likely that many alleged exposés rest upon this foundation, doing serious injustice to the psychic. When Mrs. E.'s control, the Hindu, manages to materialize a wire in such a way as to make it obvious that the wire was brought into the séance-room secreted in the psychic's shirt-waist (*Jr. Am. S. P. R.*, 4: 65-68), it is entirely possible that the control put it there during Mrs. E.'s waking state, by causing her to perform the action automatically and thus unconsciously. Sometimes there is a residuum of æsthesia for automatic phenomena, which, however, is wholly inadequate to acquaint the subject with the nature and extent of the phenomena: Miss Burton, in her normal state, says that "when the trumpet is in use during the 'independent' singing, whistling, and speaking, she cannot speak when addressed without an interruption of the physical manifestation . . . that she is not conscious of being used except for a feeling of constriction about her throat when the singing and whistling are in progress." (*Jr. Am. S. P. R.*, 3: 707). There is indeed sensibility in the anæsthetic parts of the body used but, as Professor James says, it exists in a secondary consciousness (Principles of Psychology, vol. I, p. 203) and is not available to the primary consciousness except by conquest through hypnosis or other psychopathic technique.

[44] Amnesia for acts of the body when the subject is in a state of distraction, emotional excitement, sleep, or trance, is well known. As Professor James says, the amnesia may be complete for the deeper states of trance (Prin. Psych., vol. II, 602), in which case the phenomena do not constitute a part of the subject's memory when he is in the waking state. This fact is abundantly illustrated by researches in hypnotism, alternating personality, and séance phenomena. B. C. A., in a secondary phase of personality, arises from bed, writes two letters, drops one on the stairs while returning, hides the other in a glove-box, goes to bed and to sleep, without contributing an item to her primary memory. (Prince: The Unconscious, 60-61). Professional literature is replete with celebrated cases:

Félida X., by Azam: Hypnotisme, Double Conscience et Alterations de la Personnalité. Paris, 1887.

Louis V., by Bourri et Burot: Variations de la Personnalité. Paris, 1888.

Léonie B., by Janet: L'Automatisme Psychologique.

Mary Reynolds, by S. Weir Mitchell: *Harper's Mag.*, May 1860, and James: Prin. Psych., vol. I, p. 381.

Ansel Bourne, by James: Prin. Psych., vol. I, p. 391.

Miss Beauchamp, by Prince: Dissociation of a personality.

See also:

My Life as a Dissociated Personality, by B. C. A.

Ribot: Maladies de la Personnalité. Paris, 1885.

Dessoir: Das Doppel-Ich.

Proceedings S. P. R.

Séance phenomena offer many illustrations: Permission was given to feel the

Further experimentation will have to determine whether the "séance personalities" have characteristic rates of tapping, power to exert force, etc., and how they compare with the psychic's normal performance; whether undisguised finger-prints of the "séance personalities" differ from the finger-prints of the psychic; whether any phenomena can be produced at all under conditions that exclude the instrumentality of the psychic's body, such as in an inclosed space. A solution of these problems will occupy the Society in the next series of investigations.

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pulse of "Lenore," one of Miss Burton's "controls"; finger and thumb marks were later revealed on the psychic's wrist (*Proceedings Am. S. P. R.*, 5:52). Although the psychic's hands were held during "independent" whistling in the trumpet, her offensive catarrhal breath was clearly detected in the trumpet (*ibid.*, p. 47); and by means of side-light the psychic was seen, during the séance, to leave her seat and lift a table, without Dr. Milne, who was holding her right hand, being able to detect that she had left her seat (*ibid.*, 257). In the photographs, secured by co-operation of the "control" "Dan," the psychic was revealed upon one occasion out of her chair (*Jr. Am. S. P. R.*, vol. IV, 56), and upon another, with a tambourine in her teeth, ready to throw it into the air for the picture (*ibid.*, 57). The investigators were satisfied that Miss Burton's "waking consciousness was honest and that her trance personality was knowingly deceiving her," (*ibid.*, 55), and the other trance personalities claimed as much, and, besides expressing their regret, kept "Dan" away for some time.

Under the heading of "'Mediumships,' or 'Possessions'," James (Principles of Psychology, vol. I, pp. 393 ff.) says:

"Whenever the secondary state is well developed, no memory for aught that happened during it remains after the primary consciousness comes back. The subject during the secondary consciousness speaks, writes, or acts, as if animated by a foreign person, and often names this foreign person and gives his history. . . . Usually he purports to be the spirit of a dead person. . . . Mediumistic possession in all its grades seems to form a perfectly natural special case type of alternate personality, and the susceptibility to it in some form is by no means an uncommon gift, in persons who have no other obvious nervous anomaly. The phenomena are very intricate, and are only just beginning to be studied in a proper scientific way." (p. 393).

He makes a confession of his belief that the "control" may be altogether different from any possible waking self, knowing facts about persons the psychic has never seen and does not know the names of, because he is "persuaded that a serious study of these trance-phenomena is one of the greatest needs of psychology" and would encourage investigation. (p. 396).

3. The Relation of the "Séance Personalities" to the Psychic's Mind.

Although, on the basis of experimental results, it is premature to discuss this question, it can scarcely be passed over in silence in this report, since it was the primary end and starting-point in the experimenter's research. It was only after sitting in the Society's weekly séances from September till March without making a step of progress in having the séance personalities take his preliminary clairvoyance-telepathic experiment with the playing-cards, that he consented to turn his attention to the phenomena at hand. Many of the phenomena have indeed been of the psychical order, sitters frequently recognizing in communications "splendid tests," but they have one and all been what may be described as of a memorial or historical character, such as have constantly led investigators to conflicting opinions concerning their supernormal nature, and for this reason the experimenter intended to leave their investigation to others who see some hope in their enterprise, and to adhere resolutely to communications concerning new facts so controlled by himself that results can be treated by statistical analysis. The parent Society for Psychical Research, in London, and the American Society for Psychical Research, in New York, with other societies abroad, are prosecuting with rare courage and skill the kind of research to which reference has just been made, which has for its end the proof of man's survival of death, and to them the experimenter is willing to leave the field clear. But the whole commercialization of the occult in this country rests upon the assumption that the "séance personalities," or the entranced occultists, or clairvoyants, or automatists, have supernormal means of acquiring knowledge of new facts in our world and are consequently competent to advise the sitter or client in important matters. It is this supernormal capacity of acquiring knowledge of new facts in our world that the experimenter proposed to investigate, and as a psychologist he will be particularly interested in the psychical processes which account for the supernormal knowledge, in case it is found, or for belief in it by the psychic, in case it is not found. In either case the relation of the "séance personality" to the psychic's mind will be a primary consideration.

This declaration of intention may be regarded as a forecast of further work to be undertaken by the Society.

For the present the experimenter must content himself with report-

ing a few of the phenomena which have come to his notice and have a bearing upon the characteristics and incidental traits of the "séance personalities" and their probable relation to the psychic's mind.

It must be borne in mind that the psychic has not only spent her time and energy freely, without pay, in the Society's research, but she has upon all occasions willingly acceded to any demands which the Society, or the experimenter in particular, has made, frequently showing sincere interest in the acquisition of a scientific proof of the truth, whatever it may prove to be, which underlies the phenomena which occur in her presence.

In all their communications the "séance personalities" evince the same interest and willingness to coöperate in this research. The records of the Society contain abundant evidence of this; but a few quotations will serve to illustrate: "Katie," the automatic voice, a frank and sweet child of about 12 years of age, said at the close of the séance of March 28th, after the kymographic apparatus had been used, "We are anxious to have the truth demonstrated." During the same day, "Dr. Truman," a dignified "trumpet" voice, said, "We are all very interested in this experiment, as much as yourselves," and "Professor James" said, "We are very pleased with the experiment." In the next séance (April 4th) "Professor James" said, "I wish I might be able to speak more distinctly so as to make a perfect record," and "Dr. Truman" at the close said, "We thank you for your painstaking interest and work." Thus it was all the way through the year's work until the measurement of "psychic force" began.

At this point apprehension became evident; three out of the five remaining séances were blanks; the "séance personalities" could not get sufficient force to carry out the program, or if they did, it was largely misdirected. Upon the supposition that the causes of failure were occult, the misadventures of the investigation cannot yet be explained; upon the supposition that they were natural, a very simple explanation suggests itself. A month before, the psychic removed some printer's ink from her fingers and from her trumpet. Since then the kymograph with its respiration and pulse curves furnished a distraction; but now, with the scales and the telegraph instrument to the fore again, there was a chance for imprints of fingers and traces of contact; the "séance personalities" had to have her "magnetize" (explore and test?) these instruments; progress in this was probably made even in the blank séances; what phenomena could not be safely attempted were not pro-

duced; what could be, were carried out with a sole determination to leave no finger-prints.

The evident apprehension of the "séance personalities" is matched by the disquiet of the psychic (showing some community of consciousness, possibly subliminal) and were it not for the evident sincerity and honesty of the psychic, it would be the simplest explanation to consider her direct and conscious agency the cause of the phenomena, for (1) *the phenomena were such as a disquieted psychic herself would produce, and* (2) *the guarded method of their production is such as she herself would employ.*

The night preceding the séance (April 11th) on which we returned to contact phenomena, the psychic is said to have been worried and to have slept but little; and just before the séance one of the sitters, who lives in the same house, mentioned to the experimenter that the psychic had passed through the physical phase of mediumship long ago, implying that it would be agreeable to have the investigation pass on to other fields, and, a few minutes later, to the experimenter's consternation,[45] she actually spoke something about finger-prints.

"Colonel Roland," a "trumpet voice" and one of the regular "controls," had already exhibited his perplexity concerning the printer's ink, in communications to President Booker at a private séance in which he was one of the sitters, by suggesting that he (sic) use plaster of paris or paraffin instead of so much black; and in the séance of April 25th, after "he" had knocked over the trumpet, he was reported by "Katie" as saying that President Booker will understand why it fell. The implication is erroneous (this fact will be used later), but it serves to reveal an apprehension that explains why the trumpet has been so seldom used, why it was handled through fabric, why contact on the scales was also of a guarded and indirect kind, and why the telegraph instrument was not further used.

The misuse of force would seem to amount to pretty good evidence of sheer "stalling" on the part of the "séance personalities." During the séance of April 25th, "Colonel Roland" levitated the trumpet, even placed it on the table, with the intention, according to "Katie," of setting it on the scales. The same amount of force applied to the scales would have been to some purpose. During the séance of May 9th the table was hauled out toward the center of the room about a foot and a

[45] Finger-prints were in view as marks of identification, and were taken of all the sitters and the psychic, before the séance of May 2.

half, and part of the way back again; the force applied to the telegraph instrument, or to the scales in the closet, would have been of service.[46]

Whether miscarriage of experiments was due to apprehension, which would have to rest upon lack of knowledge, or upon an inclination to frustrate rather than to aid the investigation, cannot be definitely determined, but the weight of probability falls upon the former explanation, since there are other instances of a curious limitation of knowledge.[47]

"Colonel Roland's" assumption (April 25th) that President Booker knew why the trumpet fell, is a case in point. Whether the trumpet had been handled during the preceding séance with a telescopic aluminum grasping hook ("lazy-tongs," the familiar old standby for producing physical phenomena at a distance from the medium in a dark séance) which failed to take hold because of a thin film of vaseline, or with ribbed-silk gloves to which the surface would be equally elusive, one cannot be certain, but that one or the other of the cases applies is the simplest explanation for the above assumption, stated during a séance when the trumpet was in perfect order and when President Booker did not in fact know why it fell.

"Professor James," although showing deep interest in the progress of the research, has upon more than one occasion, when the experimenter asked him a question of psychological or technical import, most unnaturally disappeared.

"William Stead," who was accustomed to come to encourage President Booker in his investigations, shocked some of the sitters beyond measure in one séance by not being able to give the president the name of his daughter, whom he left in charge of Julia's Bureau.

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[46] One is forcibly reminded of the continued failure of Eusapia's "control," "John King," to take advantage of his opportunities when scientific instruments were used; and of the classical case of substitution of one kind of phenomena for another, which is a constant marvel to the enquirers into Zöllner's grounds for belief in the occult. Zöllner (Transcendental Physics, London, 1880, pp. 97-113) asked that (1) two solid rings of different wood be interlinked; (2) the twist in two snail shells be reversed; (3) a knot be tied in an endless cord cut from a bladder; (4) a stub of a paraffin candle be put in a hollow glass ball. Slade substituted: (1) placing the two rings on the jointed leg of a stand; (2) making the snail shells appear on a slate he was holding under the table; (3) tying knots in a cord that had two ends, so as to suspend the endless cord from it. Zöllner was elated; his "tests" were improved!

[47] The alternative of "poor conditions" is scarcely worth considering, since the phenomena called for are of the same class as those produced, and are as simple and easy to execute.

Near the end of the séance of March 28th, "Katie," speaking apparently for "Dr. Hodgson" and "Professor James," who had been watching the instruments, said they thought that neither of the writing-fingers had recorded during the independent speaking, whereas the respiration records were complete. On May 2d, she reported that "Professor James" had used his right hand in exerting force on the scales, whereas the impression on the lamp-black indicates a small left hand, fingers folded in and thumb extending slightly to the right. (See Figure 18.) And during the séance of April 18th, "she"said that "Sir William Crookes" was present, so "Dr. Truman" informed her, whereas the noted scientist is still among the living.

The telegraph instrument has given us inexpert tapping, but no code. The operating, said to have been done by "Phillips," the wireless operator who went down on the Titanic, seems to show that he loses his knowledge of telegraphy when he becomes a "séance personality."

This limitation of knowledge and astounding stupidity of "trance personalities" is, of course, familiar to readers of reports of investigations in the proceedings of the two societies for psychical research, and elsewhere; and is explained, according to investigators, by the fact that the discarnate "personality" on his side must enter an abnormal state, as the "psychic" does on this side, in order to communicate at all,—even then perhaps only through the psychic's usual "control,"—and that much practice is needed in order to purge these communications of intellectual chaos.[48] But this tremendous limitation is not recognized by "sitters" who frequent séances; is not fully recognized, I suspect, in our own Society.

[48] Sir Oliver Lodge records in his "Survival of Man" (p. 292) an interesting instance of mental blindness on the part of the Myers control, communicating through Mrs. Thompson, at Edgbaston, February 19, 1901. Lodge had just suggested: "You remember the S. P. R." Control: "Do not think that I have forgotten. But I have. I have forgotten just now. Let me think. . . . They tell me it was my best love, that society. They will help me. . . . I am going to talk to you clearly and very distinctly in April. I do not know my mother's name now."

Professor Hyslop writes that he has all but abandoned the theory of communication that involves an abnormal or trance state in the discarnate personality; he is inclined to think that "the pictographic process of communication and the half mechanical conditions for letting messages through may account for all the appearances of trance or dream state in the communicator." This view he developed in the *Proceedings Am. S. P. R.*, 1912, vol. VI (*vide*, ch. II, Difficulties of communicating, pp. 48 ff.).

Is it possible that the "séance personalities" are limited in their knowledge to the content of the psychic's mind?[49]

It was the purpose of the experimenter to discuss at the last séance (May 9th) somewhat in detail with the "trumpet voices" the principal results of his investigation, and get their explanations, and their suggestions for further work. But the "trumpet voices" did not come, and

[49] It is well known that "messages" delivered in trance in automatic voice or automatic writing, and that auditory and visual hallucinations (voices and visions) perceived by a 'sensitive' often reproduce experience, sometimes much elaborated, which the subject can at times identify, but also which he often cannot recognize for the reason that it has been forgotten, or was a so-called unconscious perception, or belonged to a secondary state of consciousness. Miss X. looks across the room and tries vainly to read the title of a strange book lying on her table; she turns to her writing and sees on the blank paper "The Valley of the Lilies," which proves to be the title of the book never seen before, but no doubt subliminally read (Myers: Human Personality, vol. I, pp. 587-8). She looks through a window and reads on the pane a newspaper notice of the death of a friend; she finds the notice in a paper containing some items she remembers having read; again, a reproduction of a subliminal impression. Miss B. looks into a crystal and sees a wood, a lake, and men, and witnesses a complex murder scene; the vision was a correct representation of a scene in one of Marie Corelli's novels which she had once read but forgotten (Prince: The Unconscious, p. 42). B. C. A., whose amnesia for her conduct in a secondary phase of personality, in rising, going down stairs, writing two letters, dropping one on the stairs when returning, and hiding the other in a glove-box, has been noticed, looked into a crystal and witnessed the whole scene; in hypnosis the experience was remembered and even the thoughts which accompanied each act were described; the vision was a reproduction of experience belonging to a secondary phase of personality (Prince: *op. cit.*, 60-1). Miss C., in hypnotic trance, narrated highly elaborated fabrications of her forgotten experience; on one occasion the spirit of a fictitious person, purporting to have lived in the time of Richard II., gave many intimate details about the Earl and Countess of Salisbury, and other personages of the time; the genealogical data were found to be correct, although they were such as could be ascertained only through critical historical research. In her normal state Miss C. could not imagine how she could have obtained this knowledge, for she was in entire ignorance of it; through automatic writing it was discovered that the facts were to be found in a book called "The Countess Maud," by E. Holt, which had been read by an aunt, 14 years previously, to Miss C. when she was about 11 years of age. (*Journal S. P. R.*, July 1906; August 1911; also Prince: *Op. cit.*, 19-20). One of Prince's subjects, "while in a condition of considerable stress of mind owing to the recurrence of the anniversary of her wedding day, had a vision of her deceased husband, who addressed to her a certain consoling message. It afterwards transpired that this message was an actual reproduction of the words of a friend quoted to her in the course of a conversation some months previously, as the words of her own husband to herself just before his death. In this vision the words were put into the mouth of another person, the subject's deceased husband, and were actually heard as an

discussion had to be undertaken with "Katie." She assured the experimenter that he has been right all along in his assumption that "Dr. Truman" and "Colonel Roland" have known all that he has been doing in the conduct of his investigation, and that they know his results; she said that they commend his effort; and that they will be delighted if the proposed report is made to the Am. S. P. R., and the projected thorough investigation with the psychic is undertaken next year.

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hallucination." (Prince: *Op. cit.*, 40). The messages received in automatic script through Mrs. Verrall are often found to be quotations from passages in English, Latin, and Greek, which she has read but forgotten (*Proceedings S. P. R.*, October 1906, ch. 12). "Subconscious fabrication" seems a reasonable explanation for the life-histories given by controls who fail to prove their identity. Is it not likely that Mrs. Piper's Phinuit is an etymological descendant of "Finné," the control of a Mr. Cooke before whom Mrs. Piper first went into trance? (*Vide,* Podmore: Naturalisation of the Supernatural, 307 ff.). Was not Mrs. "Smead's" "Harrison Clarke," who said he fought at Shiloh in the 125th N. Y. Regiment, and who later confessed that Clarke was not his real name and explained that he deserted the N. Y. Regiment and joined one that actually appeared at Shiloh but refused to give further particulars, a psychic fabrication? (*Vide,* Hyslop: Apparent subconscious fabrication. *Journal of Abnormal Psych.*, 1906, 1:206 ff.).

Morton Prince, who has much experience with spontaneous and induced states of secondary consciousness, says:

"The reproduction of subconscious perceptions and forgotten knowledge in dreams, visions, hypnosis, trance states, by automatic writing, etc., is interesting apart from the theory of memory. Facts of this kind offer a rational interpretation of many well-authenticated phenomena exploited in spiritualistic literature. Much of the surprising information given by planchette, table rapping, and similar devices commonly employed by mediums, depends upon the translation of forgotten dormant experiences into manifestations of this sort." (Prince: The Unconscious, 59).

When "Annette" gives the spirit message through Mrs. Holland's automatic script, "Tell her this comes from the friend who loved cradles and cradled things," how can one be sure that it is not a slightly elaborated reproduction of the words of a letter received 20 years previously by the automatist from a friend of Annette's, quoting from the latter's will: "Because I love cradles and cradled things"? (*Proceedings S. P. R.*, 1908, 21:288-9; also, Prince: *op. cit.*, 22).

To prove that "messages" are not reproductions, simple or elaborated, of experience in some phase of the psychic's personality, the Society is evidently under the necessity of controlling the facts to be reproduced in some such way as they are controlled in the experimenter's card experiment. Statistical analysis would be capable of determining whether any of the "séance personalities" are independent of the psychic's mind, and would throw immediate light on the processes employed in an unrecognized means of acquiring knowledge. Until this step is taken, all "spirit messages" are likely to be regarded by the world at large as limited to the class of "messages" already known to be reproductions or elaborations of the psychic's own experience.

The experimenter was and is very grateful for this assurance, for he was beginning to feel that his frank assumption of full knowledge of the course of investigation, on the part of the psychic's "controls," was not justified. All now understand each other; everything has been open and above-board between the experimenter and the "controls." Their courage and "fanaticism for truth" are equal to the experimenter's, for although the results so far seem to show that the "séance personalities" speak with the psychic's vocal organs, that they effect physical phenomena with her hands, and that their knowledge is possibly limited to the contents of her mind, they desire that the experimenter shall put this evidence on record, and shall follow it up with thorough-going investigation next year to establish fully and clearly the *exact* truth.

The implications in this report the experimenter believes to be unavoidable upon the basis of the facts in his hands at the present time. He remains open-minded, however, and will not ignore good evidence for the occult phenomena of "independent" voices, levitation, personalities; indeed, he is even anxious for the sake of the interest of his friends in the Society, to seek and find such evidence. If there are such occult phenomena, he believes the present program of the Society's experimental section, involving the use of scientific instruments, is adequate to place them incontestably before the eyes of science.

The experimenter has already expressed his high respect for the psychic, and he wishes to attest the sincerity of the sitters in the Society's séances, their evident high motives in contributing time and money to an investigation which in its nature must at times have grown tiresome, and their confidence in the experimenter who was frankly sailing an unsounded sea. He commends their harmony and orderliness, which insured the psychic from the dangers of pocket flash-lights and of "grabbing" in the dark. He trusts that they, and the other members of the Society, appreciate the seriousness of the import of the experimental results so far obtained, and he hopes that they will give unreserved support to the proposed thorough-going scientific investigation next year in order that this import may be either revised or verified according to the facts found.

Perhaps a paragraph on the principles of interpretation of phenomena should be offered here for the consideration of sitters who have not had formal scientific training. This Society is a *research* society, and its existence can only be justified by the use of the methods of investigation common to all research—scientific methods.[50] These involve

[50] The scientific method ordinarily involves the use of instruments to extend

(1) experiments for the production of simple or selected facts; (2) interpretation of causes of these facts according to the "law of parsimony"; (3) the testing of the interpretation by new experiments; and so on. Now, interpretation of the causes which produce our facts must, in the beginning, be thoroughly natural, recognized everywhere in the scientific world; only when we get phenomena that cannot be so interpreted are we warranted in revising the limited interpretation. We cannot start out with the assumption that there are "independent" voices, levitation, personalities; we must start with the assumption that these phenomena are only *apparently* independent, if we are ever to get a proof that they are *actually* independent, and if we court the attention of the scientific world for our psychical research.[51]

The experimenter believes that the scientific man will read with approval the Society's first report. He cannot but appreciate the fact

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the normal powers of observation, which are notoriously fallible under the conditions of dark séances, that not only exclude vision but offer sound stimuli of minimal intensity and facility for hallucination of touch perception, to say nothing of wearying the attention by long sittings and misdirecting it by the production of undetermined phenomena. A little reflection will suffice to recognize the value of the use of scientific instruments to man in his effort to understand the forces about him and to subject them to his control. Sunlight appears to be homogeneous; the prism spreads it out into the spectrum colors for each of which the wave length and frequency have been measured. The earth looks flat; its shadow on the moon, the telescopic view of a sailing vessel at sea, and circumnavigation, show it to be spherical. The microscope reveals the malarial parasite in the blood, and also in the stomach-walls of the anopheles mosquito responsible for the patient's infection, dispelling the superstition that inhalation of the miasma of the swamp is the cause of the fever. The chronoscope, by measuring time in thousandths of a second, enables us to learn that a nervous "current" is propagated at a rate less than 200 feet a second, and that it cannot be of the nature of an electric current or of light, which travels 186,000 miles a second, or of sound that travels 1,100 feet a second. Scientific instruments have made possible our special sciences of astronomy, chemistry, physics, biology, medicine, physiology, psychology, etc., and have made our world more intelligible and a safer and a more comfortable place in which to live. Science in general is the organization of our observations thus facilitated, and may be regarded as an extension of our common sense, analogous to the steamship and steel rails as extensions of our power of locomotion, or to the telegraph and the telephone as extensions of our power of communication.

[51] This demand arising from the "law of parsimony" is that our explanations or interpretations, besides being simple, must be consistent with known causes. It is only upon the leverage of this provision that proof of new phenomena can be forced into scientific recognition. The decisiveness with which it is established corresponds to the severity of the criticism it has successfully withstood.

that the Society is proceeding *scientifically,* and the experimenter predicts that he will be waiting with interest for the Society's second report. Having thus obtained his attention and respect, all the Society needs to do to prove the existence of unusual or supernormal causes is to show by records of properly controlled scientific instruments that they occur, and occur regularly under definite conditions.

It should be definitely recorded, for the Society and the world, that the obligation of producing phenomena which cannot possibly be explained by a *direct* and a not unfamiliar use of the psychic's body and mind rests upon the "séance personalities," so long as the Society, through its officers, its investigation committee, its experimenter, and its psychic, is providing séance conditions under which *any* phenomena at all take place.

The experimenter wishes to express especial obligations to the president for many hours' assistance in this and related psychical research, to the secretary, for faithful service in recording his dictations during the Society's séances, and to the treasurer for valuable assistance in the acquisition and care of apparatus used in our laboratory.

Respectfully submitted, May 28th, 1914.

JOHN E. COOVER,
Experimenter.

The Society is grateful to the experimenter for his careful work and wishes to commend the scientific method of investigation for its future work. It wishes to emphasize the facts, however, that Dr. Coover does not offer his explanations as final, and that some of the members of the investigation committee candidly differ with them on the basis of the present data, as will be pointed out below. They are glad to have his explanations expressed as they are in order that the world of agnostics can see that naturalistic interpretations are being considered from the very beginning of the Society's work; our proof, then, for unusual causes of séance phenomena cannot fail to be valid.

In taking up *seriatim* the points upon which some members differ with the explanations offered in the foregoing report, the Society wishes to put on record alternative explanations which these members believe to be equally compatible with the data.

(1) The "independent" voices may, indeed, be produced by the use of the psychic's vocal organs, necessitating a sympathetic activity in her throat; but the manner may be yet undiscovered. An independent vocal apparatus may be built up from hers and materialized to operate at a distance from the psychic's. The vocal organs of the psychic may even

produce some sound, but on this explanation of the production of independent voices it would be negligible. Some evidence to support this theory was quoted by the experimenter: The physicians felt with their hands a throat two feet distant from Miss Burton's throat;[52] and while examining the psychic's throat, during independent singing and whistling, they found slight sympathetic activity, but could hear no sound at her mouth.[53] The Report stated also that these phenomena occurred when a handkerchief was bound over her mouth. With Mrs. Blake the sound came from the trumpet in broad daylight, when only her fingers touched it.[54] It could scarcely be produced by her vocal organs without detection, even assuming that the trumpet acts as a resonator.

(2) The trumpet may be handled by a materialized hand cast in the astral mold of the psychic's arm; this independent hand may be intimately connected with the psychic's hand in that the psychic's energy is used for its movement; it may be becomingly draped. The writing-fingers on the apparatus might have been pulled out of place by this hand manipulating from the psychic's hand; the lamp-black might have been removed by the touch of the drapery, the ink would naturally be carried from the telegraph instrument, and the lamp-black from the scale-cover, to the trumpet, and finally the ink might be left on the psychic's fingers in the process of dematerialization. Some evidence quoted by the experimenter supports this explanation: A noted investigator saw the materialized hand of a "control" six feet away from the left hand of our psychic, Mrs. Key;[55] and Sir Oliver Lodge and others felt the patting, pulling, and pushing of a hand that could not possibly have been Eusapia's. Besides, investigators, Hamlin Garland and Flower, among others, have, as quoted by the experimenter, found the bonds of their psychic, Mrs. S., intact after the manipulation of objects, including the trumpet, which were known from measurement to be beyond her reach.

(3) Blank séances and the alleged misuse of force are recognized by all to be frequent, but they need not be due to ignorance on the part of the "séance personalities"; if our alternatives suggested above are reasonable explanations of the data at hand, there must be many complex conditions to be obtained for successful phenomena, about which as yet we know nothing or next to nothing. These not being under our con-

[52] *Vide*, p. 507, *supra*.
[53] *Vide*, p. 520, *supra*.
[54] *Vide*, p. 506, *supra*.
[55] *Vide*, p. 504, *supra*.

trol, it is more or less accidental when they are fulfilled. The experimenter himself has quoted a marked case of "mental blindness" occurring in Sir Oliver Lodge's investigations,[56] which excuses any errors or limitation in knowledge on the part of our "séance personalities."

(4) The apprehension which the experimenter sees may be in part real, due to the psychic's learning of phenomena that do not seem to be regular and which she does not understand, and in part fancied, due to chance sequence of phenomena. There may be a reason which we do not yet understand why the scale-covers were knocked off or cleaned off or pressed upon through fabric. As long as we do not more fully understand the conditions necessary for the production of physical phenomena, the door must be left open for the alternative of this unknown reason. The sequence of events in this case would only *appear* to show a determination to leave no traces of contact.

(5) The Society is aware of the phenomena of so-called "secondary personalities" reported by eminent medical practitioners, and it thinks that the hypothesis of spirit helpers and spirit possession [57] furnishes a rational explanation of the facts of the reproduction of the subject's forgotten or subconscious experience.

With these alternative explanations on record, the Society commends the program suggested by the experimenter for its next series of investigations as being likely to throw needed light upon the questions specifically raised in this report.

EMMA L. HUME, Secretary,
3968 Sacramento Street,
San Francisco, California.

The projected experimentation did not take place the following year, owing partly to the fact that the psychic was not in her usual health and partly to another fact which may have contributed to the first —the opposition of certain professionally interested friends who professed to suspect that the investigation was a projected exposé the publication of which was intended to win notoriety or laurels for the experimenter. The psychic, however, had occasion to realize the importance of the work in hand and the great service to humanity she could

[56] Footnote 48, p. 539, *supra*.

[57] Did not the noted English psychologist, W. M'Dougall (The case of Sally Beauchamp. *Proceedings S. P. R.*, 1905-7, 19:430), consider "Sally," whom Prince (Dissociation of a Personality) suppressed as an alternating personality, to be a possessing spirit?

render in coöperating with the Society in carrying out the proposed program, for, if she were herself in doubt on account of some of the unexpected findings of the preliminary report, she was given reassurance by the highest authority on psychical research in America in the following letter:

THE AMERICAN INSTITUTE FOR SCIENTIFIC RESEARCH
SECTION B
AMERICAN SOCIETY FOR PSYCHICAL RESEARCH
NEW YORK
519 WEST 149TH STREET

November 22d, 1914.

Dear 'Mrs. Key':

I have just read and sent to press the report of the California Society for Psychical Research and I write to say that I have been much interested in it as a good piece of work. It did not result in as positive proof as the experimenters desired for spiritistic agencies, but that does not derogate from its scientific character and I shall value the paper very much. The evidence that your organism was affected in the phenomena observed does not in the least militate against spiritistic influences. It only limits the evidence for such influences. I myself have held and taught for years that the organism and subconsciousness of the medium is bound to modify the results and to be a coloring factor in the genuine as well as in other phenomena. In all my work I have large amounts of non-evidential incidents and statements as well as muscular movements that the skeptic can refer to subconscious action. That is an inevitable event in the work and it will not help the spiritistic interpretation to deny it. I have seen many cases in which the phenomena of the subject were worthless as evidence, but the moment I took the subject to another psychic and had the necessary cross-references it completely altered the case. What we had to treat as subconscious in the subject, without this confirmation, became evidential when obtained through another psychic. Witness the cases, Thompson-Gifford, DeCamp-Stockton and Rogers-Abbott. In each taken alone I did not have the evidence that the alleged person was present and influencing the subject's experiences, but the moment I got the same facts through another psychic who knew nothing of them, the spiritistic theory became as legitimate as any other. It is the same with actions that are not evidential. I have just completed a series of experiments where this is true. We have to assume that the organism and subconsciousness of the medium will always be a factor in the genuine phenomena. What we want is indubitable evidence and later we can extend the explanatory power of spiritistic influences.

Very sincerely,

(Signed) James H. Hyslop.

It is probable, therefore, that the responsibility for the temporary interruption in the investigation rests wholly upon the shoulders of the psychic's misguided advisers, who at one time charge that scientific men will pay no attention to the phenomena of spiritualism and at another when a most favorable opportunity is offered strongly oppose the scientific study of the phenomena, and who seem to have had less faith in the scientific verification of the phenomena than had the principal members of the investigation committee who bravely and stoutly supported the program.

The Society, consequently, has had to confine its attention to the investigation of such subordinate phenomena as are presented from time to time, hopefully awaiting an opportunity to carry out its complete program.

This opportunity may reasonably be expected to present itself soon, for the longer it is voluntarily delayed by those who can produce the phenomena, the stronger will the presumption grow that commercially interested psychics are willing to have their spiritualistic clientele believe in phenomena that they themselves fear to have properly inspected. And even were a sort of mediums' protective association to make it a common cause to withhold the awaited opportunity the presumption would apply so particularly to the leaders of that organization that the more prominent spiritualists, men of influence and property, who support the program and regret the delay, would no doubt find a way of overcoming the opposition.

Two important factors in the present situation which should appeal favorably to psychics may be pointed out:

(1) At the beginning of its existence the Society was unknown and, although men of prominence in both science and business were connected with it, was not in a position to confer honor through coöperation upon a prominent psychic. The hope of the Society for authoritative recognition was fulfilled upon the publication of its preliminary report: It is now recognized by the American Society for Psychical Research as an independent affiliated society whose reports of scientific work are acceptable for publication in the American *Proceedings*. The honor of coöperating with a society of recognized scientific standing awaits the psychic who places herself at the disposal of the California Psychical Research Society.

(2) The program of the Society has been approved, with and without qualification, by authorities in psychical research at home and abroad.

In correspondence with the experimenter, consequent upon the publication of the Society's Report, Sir Oliver Lodge says:

> Reflex or sympathetic actions on the part of Eusapia's organization were a frequent experience at times when the control was quite perfect. In so far as "physical phenomena" are extensions of physiological processes this is only natural, though manifestly it may be regarded as suspicious. Every kind of suspicion was felt and guarded against: indeed, that was the object of the sitters. But I quite agree that physiological experiments and apparatus ought to be used in a judicious manner at the appropriate time, and that the results so obtained may ultimately be instructive.

Dr. James H. Hyslop wrote:

> Your methods of experiment I thoroughly agree with. I should have employed similar ones if I had had the laboratory and apparatus, but I have not had them and I have had to confine my work to what could be done without apparatus. . . . For unifying the world the laboratory method is indispensable. I wish we had a laboratory for the work in that direction.

Mr. Hereward Carrington of New York, wrote:

> As to the Report: I think it is very excellent in the main, and is exactly the sort of thing I had hoped to do, if we get our laboratory here going. I think you've done a good piece of work, and one which ought to please and interest the scientific man. [From reported evidence apart from the laboratory experiments] I am even yet inclined to believe . . . that she [the psychic] has produced some genuine manifestations. At the same time, I think your Report is impressive. If she is willing to continue giving her services . . . this is surely a good sign of her conscious honesty, at least, which you seem to accept.
>
> . . . The main problem [is] . . . whether the physical phenomena were produced supernormally or no. . . . Your program sounds good, and I only hope you can follow it out.

The importance of the relation of the "séance personalities" to the psychic's mind is recognized everywhere in psychical research, and the part of the program which provides for the development of a sure method of proving the independence of the "séance personality," has received hearty indorsement.

Under these conditions it is unlikely that any group of interested persons would risk for long a concerted action to prevent the carrying out of the Society's program,—an effort patently calculated to defeat the purposes and blast the hope of all those who are endeavoring to place the phenomena of spiritualism upon a basis as firm as the facts of science, and equally calculated to block the *distinterested* pursuit of truth.

APPENDIX E.

CATALOGUE OF LITERATURE IN THE LIBRARY OF LELAND STANFORD JUNIOR UNIVERSITY RELATING DIRECTLY OR INDIRECTLY TO PSYCHICAL RESEARCH.

BOOKS.

(The books indicated by an asterisk (*) were purchased upon a special fund which has been supplied, from year to year, by Mr. Thomas Welton Stanford, of Melbourne, Australia.)

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ERRATA.

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10, line 20, "son" should read "person"
12, line 10, "Mr. and Mrs." should read "Mr. or of Mrs."
34, line 6 (from bottom), "Baird" should read "Beard"
74, Table XXX, line 1, "200" under "Total" should read "220"
78, line 3 (from the bottom), "12:9-11" should read "11:9-11"
81, note 147, the dash under "log γ^2" belongs under "log log e"
84, line 17, "$\delta^2 = .051$" should read "$\delta = .051$"
95, line 16, "binomial theorem" should read "binomial"
96, note 167, formula should read

$$\frac{n!}{k!\,(n-k)!}\lambda^{k}\,(1-\lambda)^{n-k}$$

97, Table XLIII, line "C R P . . . C R" belongs below line "Below x . . . Exactly x," as in Table XLIV, p. 98
110, note 179, formula should read

$$\Delta=\sqrt{\Sigma\frac{\delta_1^2}{y}}$$

as in note 138, p. 315
110, line 4 (from bottom), "(γ)" should read "(y)"
165, line 22 should follow line 25
181, line 18, "$2p$" should read "$.2p$"
275, line 2 (from bottom), "96-104" should read "95-104"
310, line 27, for "expectd" read "expected"
322, line 6, "or drawing of Balls" should read "and drawing of Balls"
393, line 10, "substitutions were" should read "substitutions was"
425, line 18, "dates" should read "data"
429, line 7 belongs below the cut
474, line 9, "which which" should read "with which"
489, line 12, "It was a triangle" should read "It was a tangle"
489, note 50 should read: "*Ibid. Cf.*, Fig. 7, p. 481, *supra.*"
490, last paragraph should be spaced off from the preceding paragraph to show that it does not continue the preceding quotation
492, line 27, "Fig. 22." should read "No. 22, (Fig. 7, p. 481, *supra*)."
498, note 97 should continue: "*Lux*, 1895, 8: Nos. 8, 9."
509, line 10, "General Roland" should read "Colonel Roland"
521, line 11, "curves above" should read "curves below"
541, line 14, "Mr. Cooke" should read "Mr. Cocke"
625, to "Crookes, W." add page "2"
626, to "Dunlap, K." add page "xviii"
626, to "Gurney, E." add page "2"
626, to "James, W." add page "vi"

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